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A Study in Reaction Time and Movement,

BY

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I. INTRODUCTION.

The problem of reaction time has already been studied by many psychologists of great merit. The various influences which lengthen or shorten reaction time have been worked out, and at first sight it would seem a most unpromising field of research. And if the attempt had been made to study reaction time alone, it is scarcely probable that anything new and important could have been brought to light. But while reaction time itself has been quite thoroughly studied, the movement by which the reaction is made has received but little attention. This neglect has arisen, in all probability, from the fact that when the problem of reaction time was being actively discussed the attention of psychologists was focused upon representative and not upon motor processes. Only of late years has movement come to the foreground, as an object of psychological study. Motor processes are being recognized more and more as important elements in conscious processes. Professor Münsterberg has even gone so far as to assert that consciousness is absolutely dependent upon the possibility of a motor discharge.[1]

The evident endeavor which asserts itself in many quarters to bring in motor processes to explain the fact of consciousness, makes it all the more important that they should be the object of experimental research. And on this account, too, a study in the psychology of movement has a value over and above whatever technical merits it may possess. Even purely physiological and anatomical studies of movement are found to have their paths of connection — long perhaps, but clearly marked — with the broader highways of philosophical inquiry. For whether the new theories are right or wrong, they will not and ought not to be laid aside, till subjected to the most searching and critical tests. And in the very nature of the case the study of movement will be of prime importance in the confirmation or rejection of any theory of consciousness which rests upon motor processes as the very groundwork of its structure.

[1] 'Grundzüge der Psychologie,' Vol. I., p. 530.

The phenomenon of reaction seems to offer peculiar advantages for this study. Külpe defines reaction as the 'answer given to a sensory impression by means of movement.'[1] Whether or not this movement comes as the result of conscious processes, or accompanies or perhaps precedes them, is a matter of discussion. Cattell persists in looking upon all reactions as a kind of reflex. He defines and analyzes the process as follows: "The reaction time is the interval elapsing before a predetermined movement follows on a predetermined stimulus. During this interval a series of physiological processes takes place. (1) The stimulus is converted into a nervous impulse; (2) the nervous impulse travels along the sensory nerve and, it may be, the spinal cord to the brain; (3) through sensory tracts of the brain to a sensory center; (4) changes occur in this center; (5) these changes are followed by a discharge from a motor center; (6) the motor impulse travels along motor tracts in the brain; (7) along the motor nerve and, it may be, the spinal cord, and finally, (8) the muscle is innervated. The process is probably an acquired cerebral reflex, not accompanied by consciousness. The stimulus is indeed perceived, but probably not before the motor impulse has been discharged. The stimulus causes two sorts of cerebral changes, the discharge of the motor impulse, and changes in the cortex, which are accompanied by consciousness. But, contrary to the views of most psychologists, we think the movement does not follow on changes in consciousness, but is stimultaneous with, or actually prior to them. What volition is concerned in the process precedes the reaction and consists in preparing the motor impulse which is reflexly discharged."[2]

Directly opposite to this analysis of reaction time is that of Wundt. According to him, reaction time is made up of the following processes: "(1) Conduction from the organ of sense to the brain; (2) entrance into the field of consciousness; (3) entrance into the focal point of consciousness; (4) the stimulation of the will, which sets free the motion registered in the central organ; and (5) conduction of the motor discharge thus

[1] 'Grundriss der Psychologie,' Leipzig, 1893, p. 421.

[2] *National Academy of Sciences*, Vol. VII., pp. 393-394.

arisen to the muscles and the increase of energy in them."[1] The process of reaction here analyzed Wundt looks upon as complete; and it is termed by him a sensory reaction. When, however, the attention is focused upon the reacting organ, a shortened form of reaction takes place which, according to Wundt, corresponds to the analysis given by Cattell. This form of reaction Wundt terms muscular, still adhering to the distinction pointed out by L. Lange in 1888.

In an article communicated to *Mind* in January, 1903,[2] Mr. W. G. Smith called attention to another factor which has a bearing upon the analysis of the reaction process. He found that with some subjects, before the prearranged reaction movement commenced, there was a preliminary movement in the opposite direction. This antagonistic movement consumed from four to five hundredths of a second and occurred in various muscles and with various movements. With some subjects it occurred constantly, with others but rarely or not at all. Physiologically he attributed this to a prior contraction of the antagonists. This was an involuntary adjustment calculated to produce a forcible movement rather than a sudden start. Psychologically, he referred the phenomenon to the dominance of the idea — first holding possession of consciousness — to hold the finger pressed down upon the key. A further consideration of this problem and its bearing on our results, will be taken up later.[3] We shall now pass on to an account of the literature bearing more particularly upon the problem under discussion.

[1] 'Grundzüge der physiologischen Psychologie,' 4th ed., Vol. II., p. 306. Cf. also 5th ed., Vol. III, p. 384.

[2] 'Antagonistic Reactions,' pp. 47–58.

[3] *Infra*, p. 55.

II. LITERATURE.

Since the question of reaction time is only subsidiary to our real problem, it will not be necessary to give in this paper an account of its rather extensive literature. References, however, to the various accounts of the literature of reaction time will be given in the bibliography. The motor discharge by which the reaction is made has been studied but little. Most of the previous investigations in this field of research have been concerned separately with one or the other of the two phenomena into whose relation we now inquire. Movement has been given a great deal of attention in studies of fatigue. The perception of movement and its accuracy have also been the subject of much research.

In a paper communicated to the Royal Society of London,[1] Dr. William R. Jack presented a study of the velocity of various finger movements for people of different pursuits and ages. No comparison of the velocity of movement with the time of reaction was made. Only the speed of movement was studied and its variation according to the age and ordinary life of the subject.

Dr. J. B. Haycraft published an article,[2] 'Upon the Production of Rapid Voluntary Movements,' in 1898. He measured the time of flexion of the middle finger and also found that the velocity of a movement checked in its initial stage was almost four times greater than that of a movement made in the ordinary way.

The literature which bears upon the relation between reaction time and the subsequent movement is not extensive. The problem was mentioned in a doctorate dissertation by C. B. Bliss, published in the 'Studies from the Yale Psychological Laboratory' in 1893. "In connection," he writes, "with these experiments where the attention was directed to a motion for which the reaction was a means, the idea suggested itself, but

[1] *Proc. of the Royal Soc. of London*, Vol. LVII., 1895, pp. 477-481.
[2] *Journal of Physiology*, 1898, Vol. XXIII., pp. 1-9.

has not yet been carried out, of having a second reaction key in place of the point on the table. Then we should have recorded, in addition to the reaction time, the time required to make a certain movement. This would probably vary from time to time, with changing mental and physical conditions. None of its variations could be attributed to influences acting upon the conscious part of the reaction, for it would be purely automatic after slight practice. This might throw some light on the relative portion of the variation which is to be assigned to the purely psychical part. Possibly it might be used instead of simple reaction as a standard for comparing different kinds of reaction time."[1] Dr. Bliss made experiments in which "the reaction consisted in touching a point on the table six inches from the key. Raising the finger from the key to make this motion broke the spark coil circuit and so only the beginning of the motion was registered. Here again the mind was mistaken in judging that the reaction time was quicker than usual."

As early as 1889[2] M. Charles Féré, who has given us a number of studies in movement, undertook to investigate the relation between the time of reaction and the force of movement. He made three sets of experiments. The first was on subjects suffering from hysteria. With these subjects there was a decided difference between the pressures which the two hands were capable of exerting separately. It was found that the hand which gave the strongest dynamometric pressure reacted more quickly than the other. Furthermore, under the influence of pleasurable emotions, which were suggested to the subjects, the dynamometric pressure was strengthened and the reaction time quickened. The odor of musk strengthened both hands considerably and notably shortened the reaction time. The second series of experiments was made on epileptics. After an attack of epilepsy these subjects suffer a decided loss of muscular power. Reaction times measured after such attacks were considerably lengthened. The third series of experiments was made on normal subjects, with the aid of a specially constructed dynamometer, which could measure the force of fifty different

[1] *Op. cit.*, p. 37.
[2] *Revue Philosophique*, Vol. XXVIII., pp. 36–69.

movements of flexion and extension of the muscles which control the movements of the hand. The reaction time was measured by the d'Arsonval chronometer. It seems that the measurements of pressure and extension were taken in separate series, since the author does not tell us of any attempt to obtain the pressure of the actual movement of reaction. He sought to investigate the relation between the force a given muscle or a set of muscles was capable of exerting and the speed with which it could react. The signal for reaction was a touch on the back of the hand, the eyes of the subject being closed. The results obtained seemed to indicate that the time of reaction was a little quicker according as the muscles involved were capable of exerting greater force. In another set of experiments the recording apparatus consisted of a Marey signal, two tambours and a tuning-fork. By this apparatus it was shown (*a*) that the time of reaction for the left hand was slower than that of the right; (*b*) that the sum of the reaction times of both hands was greater when the attempt was made to react simultaneously, than when they reacted successively. M. Féré considered a similar conclusion as established, namely, that the sum of the pressure of the two hands is greater when each acts successively than when both act together. From these two results M. Féré deduced the law that the rapidity and amount of nervous discharge are greater according as the outlets are less numerous.

In 1889, Dr. J. Orschansky touched directly upon the relation between reaction time and the subsequent movement, while investigating the nature of inhibition.[1] The amount of muscular movement in reaction, he says, is composed of two factors if we abstract from its velocity. These two factors are the intensity of muscular contraction and its amplitude. He determined the intensity of muscular contraction by making the *masseter* react against springs of three different strengths. He merely tells us that the amplitude of the contraction was recorded by the Marey tambour. But just how the extent of movement was limited — whether mechanically or by accident or by the subject's judgment — is not stated. He found that the most favorable conditions for a quick reaction were present when either the tension of the springs was at a minimum and the

[1] *Archiv für Physiologie* (*du Bois-Reymond*), 1889, pp. 173–198.

amplitude at a maximum, or the tension at a maximum and the amplitude at a minimum. This he explained by saying that the quantity of tension in the apparatus acted as a sensorial stimulus for the attention, while the idea of the extent of movement was an element in the motor representation which preceded the reaction. If, however, neither tension nor amplitude was at a maximum or a minimum, it was found that the reaction time was lengthened. This was explained on the ground that in such cases the attention had to deal with more complicated conditions. In the former case the psychic act of reaction consisted of one factor at a maximum while the other was excluded. In the latter case the attention had to deal with both factors. It is to be regretted that Dr. Orschansky did not give us a more detailed account of his experiments and furnish us with data concerning the number of experiments and their mean variation. For it is hard to pass judgment on the value of the results from the paper in its present form.

In 1892[1] M. Ch. Féré studied the relation of reaction time to the weight lifted in reacting. He arrived at the following conclusions:

For one and the same subject the reaction time is longer according as the weight to be lifted is heavier — provided that the weight is not known beforehand.

When, however, the weight to be lifted is known to the subject beforehand the length of reaction time does not vary regularly with the weight, but with the capability of the subject to adapt his attention.

Deceiving the subject as to the weight to be lifted affects the reaction time. If he expects a light weight and has to move a heavy one, the reaction time is lengthened and the ascent of the curve of movement is very oblique. If the reverse happens, the reaction time is shortened and the ascent of the curve of movement is vertical.

The time of reaction for letting a weight fall is shorter than for lifting a weight.[2]

[1] *Comptes Rendus de la Société de Biologie*, 9th Series, Vol. IV., 1892, pp. 432–435.

[2] This result is not against that of Dr. Orschansky, for antagonistic muscles are involved in raising and lowering a weight. In Dr. Orschansky's experiments merely the contraction or relaxation of the *masseter* was involved.

The heavier the weight the longer the reaction time of relaxation if the weight has not been supported long.

If it has been supported from one to two minutes, the longer it has been supported the longer will be the reaction time. After the onset of fatigue, however, the reaction time is shortened. M. Féré explains this last result by saying that the subject has already commenced to relax when he hears the signal.

The results of M. Féré seem to be at variance with one of the conclusions of Awramoff, whose work is mentioned below. This writer found that the quicker reaction time is obtained when the heavier weight is to be lifted. He made no attempt to harmonize his results with those of M. Féré. But some explanation of the difference may be sought in the rapidity with which Awramoff's experiments followed one upon another.

In Volume XVIII. of the *Philosophische Studien*[1] Dobri Awramoff published an article entitled 'Arbeit und Rhythmus.' The second part of this article is a study of reactions made by lifting weights. The subjects lifted a weight of three kilograms in most of these experiments. The apparatus used was an ordinary ergograph. He found that when the signals to react are given rhythmically (*e. g.*, every two seconds) the subject reacts more quickly than when the signals come at irregular intervals. He explained this phenomenon on the ground of the distinction between muscular and sensorial reaction. Irregular intervals between the stimuli make it more necessary for the subject to be on the lookout for the signal. But regular intervals occasion the subject to keep up a motor preparation for the movement. He also found that a rhythm of one second gave rise to a quicker reaction than a rhythm of two seconds. The reaction time for raising a weight of five kilograms was shorter than that for raising a weight of three kilograms. The reaction times being measured by the graphic method, a curve of the movement was obtained from which the author was able to arrive at several conclusions. The length of time the weight is held up is longer for non-rhythmic than for rhythmic reactions. The height to which the weight was lifted was greater for non-rhythmic than for rhythmic reactions. It was also claimed that the form of the curve was indicative of individual peculiarities.

[1] Pp. 515-562.

As a preliminary to a more complete paper on the study of temperament, Prof. E. B. Delabarre communicated an article 'On the Force and Rapidity of Reaction Movements' to the Psychological Review in November, 1897.[1] The object of the experiments was to determine the individual peculiarities in the subject's manner of reacting. The subject's attention was therefore turned away as far as possible from the reacting movement itself so as to leave it perfectly natural and spontaneous. The apparatus used recorded the reaction time, the pressure of the reaction movement, and the duration of that part of the movement during which the pressure of the reacting muscles increased in intensity. It was found that the ratio of pressure to duration time tended in any one series to constancy. Several individual peculiarities were pointed out, but no fixed relation was found between reaction time and either the pressure or the duration of the pressure.

[1] Vol. IV., pp. 615–631.

III. STATEMENT OF THE PROBLEM.

The results just mentioned from Professor Delabarre and his associates were just what we should antecedently expect. If the force of movement is left to accident and individual temperament, there is no *a priori* probability that a constant ratio will be found between the time of reaction and the force of the subsequent movement. It was essential to the study of temperament, however, that the force of movement should not be predetermined by special choice. But over and above any question of individual temperament, there is another which is presented in a study of the movement by which a reaction is executed. This movement is the manifestation of the motor discharge by which the reaction is executed. It is the immediate result of the efferent and central processes in reaction, and can perhaps be made to throw some light on several problems in general psychology. But in order that this may be done, the motor discharge must not vary with the accidents of temperament and transient conditions. For the large accidental variations taking place under such conditions would altogether obscure the lesser fluctuations which might be due to disturbances of the attention, interpolated psychical processes, sensory stimuli, etc.

Some kind of constant must evidently be the basis of our comparison, and in this case the only constant possible is the subject's maximum effort. One is fairly sure of sending the maximum discharge from the motor center; but even after long practice it would scarcely be possible to be sure of dividing that discharge into any constant fraction. Whether or not our maximum effort is a merely subjective constant varying in its objective manifestation from moment to moment, is a problem which experiment alone can determine. But at all events, the subject's maximum effort should be constant to serve as our basis of comparison. The factor most likely to cause its objective manifestation to vary is fatigue. Fatigue would therefore enter into the results as an undesirable factor. It would obscure those

lesser variations which might be due to disturbances or stimuli whose effect we seek to measure. In order, then, to eliminate this undesirable factor as far as possible, it is necessary to study the maximum speed of an unresisted movement rather than the maximum force which any given muscle or set of muscles could exert in a single contraction. The maximum rapidity of a movement is just as much the index of a motor discharge as its maximum force, and in all probability it represents a simpler discharge — approaching as near to a unit discharge as any voluntary contraction can possibly do. For these reasons it was decided to take the maximum speed of an unresisted movement, made in response to a stimulus to react as the basis of our study rather than the stronger force of movement. The fundamental problem of the whole inquiry thus took on this form: *When the attempt is made to react with the quickest possible movement, is there any relation between the time of reaction and the speed of the movement?*

The first step in the solution of this problem is evidently the choice of a movement for reaction which will serve best the scope of the present investigation. In previous investigations of reaction time, a number of movements have been selected by which the subject responded to the incoming stimulus. M. Féré, as we have said, investigated the reaction time for each of the fingers. Cattell has studied that of the finger, wrist, forearm, shoulder,[1] and foot.[2] And in some pieces of work sufficient attention has not always been paid to stating by just what movement the subject reacted. Cattell's results showed a difference between the reaction times of the wrist, forearm and shoulder. And when a subject is told to press down upon or to raise his finger from a telegraph key there are various muscles and combinations of muscles which might be brought into play unless some precautions are taken to insure the same movement each time the subject reacts. While the lack of such precautions in ordinary experiments on reaction time would be only a minor defect, it would certainly be more serious in a study which

[1] 'On Reaction Time and the Velocity of the Nervous Impulse,' *Nat. Acad. of Sciences*, Vol. VII., p. 410.

[2] *Loc. cit.*, p. 404.

investigates the relation between reaction time and the speed of movement by which it is executed. For purposes of the present work it is of prime importance to secure the same movement throughout the entire series of experiments, unless special reason intervenes for changing its extent or abandoning it for some other.

The method of experiment suggested by Dr. Bliss for the investigation of this problem would have been open to several objections. In the first place, the path of movement would be subject to endless variation. In lifting the hand from one key to another the arm might be raised six inches or more, or pass almost in a straight line from one key to another. In the second place, varying combinations of fingers, wrist, forearm, and shoulder might be called into play, in order to touch the second telegraph key. And besides, there would be present a disturbance of a psychological character in the care that would have to be taken lest the second key should be missed.

One desirable feature in the movement is that its possible path should be of considerable extent, so that it can be measured with accuracy and also be lengthened or shortened should the experimenter so desire. But in itself there is no reason why one movement has any particular advantage over another — so long as the same movement is secured throughout. The speed of the subsequent movement is studied as an index of the motor discharge, which passes down from brain to muscle. Different cortical areas will of course be involved when different movements are made. But the motor discharge of one cortical area is just as good for our present purpose as that of any other. But it would be fatal to change, without knowing it, in the same series of experiments from one cortical area to another. This would subject the experiments to hidden variations and render trustworthy interpretations impossible.

After some consideration the movement decided upon was an outward rotation of the *humerus*. In this movement there is no work done by lifting the arm from one plane to another, and the apparatus used supported the arm, so that, as far as possible, the muscles were free from all resistance. This eliminated any complications which might arise from fatigue. Nor did the results obtained indicate they were in any way influenced by the onset of fatigue.

IV. DESCRIPTION OF APPARATUS.

The apparatus devised for the measurement of this movement was quite simple. A brass rod 53.2 cm. long and 14 mm. in diameter was pivoted at one extremity on a metal support which rested on a large semicircular wooden base of about the same radius as the rod. Near the pivoted end of the rod there was a semicircular brass rest (lined inside with felt) to support the elbow. Toward the other end of the rod there was an upright handle which the subject grasped with his right hand. This handle could be moved up or down the brass rod and fixed by means of a screw, to suit the length of arm of the subject under experiment. In one corner of the wooden base there was a metal post, through which an electrical contact was made with the lever, if the circuit were elsewhere closed. So slight was the friction of the pivoted lever that the least movement of the subject's arm sufficed to break this contact. This occasioned some trouble at first, for the subject unwittingly broke and remade contact several times before the signal to react. This could have been obviated by instructing him to keep the lever lightly pressed against the metal post until the signal to react. Such a scheme, however, would have introduced some sort of error. For at the beginning of the movement the antagonistic muscles would have to be relaxed and their resistance overcome. The amount of this resistance would also vary in each experiment, according as the subject pressed more or less heavily against the post. Accordingly a latch was devised to obviate the difficulty. A V-shaped piece of brass was pivoted on a metal post stationed at the edge of the wooden base. A steel spring was hung between a shoulder on this piece of brass and a hook on the wooden base. When the lever was at the post, one arm of the V-shaped piece of brass rested against its exterior side and by means of the spring the lever was kept in contact with the post. The resistance exerted by this spring to the start of the movement was too slight to be felt by the

subject, but it sufficed to keep the contact made until the movement of reaction. Only a part of the movement made was recorded, because its absolute length was subject to accidental variations, being stopped here or there arbitrarily by the action of the antagonistic muscles, as was shown by graphic curves of the movement. The angle of movement made in nearly all of these experiments was 20°. The electric contact being broken when the lever was moved away from the metal post, it was closed again when it passed over the twenty-degree mark, by a specially devised contact apparatus. About 6 cm. from the end of the lever a clamp held a rather stiff steel wire, which passed downward about 15 mm. and then bending at a right angle extended as far as the edge of the wooden base. This piece of flexible wire served to close a contact apparatus or key, situated on the edge of the wooden base about twenty degrees from the metal post. Several different keys were tried. That finally adopted consisted of a semicircle of thin sheet brass filed down at the ends and pivoted at the center of the circle. The single radius passed upward from the pivot to the semicircle and was thence continued about 2 cm. This upward projection was covered by a piece of soft rubber tubing, which served as insulation and also to deaden the sound. When the piece of steel wire struck the rubber it threw this semicircular piece of brass over until it was clasped by two jaws of spring-brass, an electric contact being thereby established and a record made of the end of the movement. Both this contact and that at the post were on the same circuit as an electromagnetic sound-hammer which gave the signal for reaction. The hammer was operated, of course, independently of this circuit and served both to give the preparatory signal and the stimulus for reaction. On breaking the sound-hammer circuit, the electromagnet was demagnetized, the hammer was pulled up by a spring and struck a bell above. This gave the warning signal. On closing this circuit again the hammer was drawn and struck a metal anvil. This stroke closed the same circuit in which the lever, above described, was connected. The sound served as a signal to react. The closing of the circuit was recorded in another room by an electromagnetic time-marker,

which wrote on a smoked drum and gave five hundred double vibrations a second. When this same circuit was broken by the outward movement of the arm, and closed again as the lever passed the twenty-degree mark, both the breaking and the making of the circuit were again recorded by the electromagnetic time-marker in the distant room. The giving of preparatory signal and the sound stimulus for reaction was done mechanically by a slight modification of the older form of apparatus which Wundt called the time-sense apparatus, and described in the fourth edition of his 'Grundzüge der physiologischen Psychologie.'[1] One of the contact-breakers there described served to break the current, thus giving the preparatory signal. A metal bar was placed on another one of these contrivances, and when the steel projection on the wheel of the apparatus reached this bar, contact was established and the rotation of the apparatus stopped. A special series of experiments was made by which this instrument was calibrated so as to measure, quite accurately, intervals of one-half second. The interval between the preparatory signal and the stimulus for reaction could then be varied at will, by placing the contact-breaker at the proper number of degrees from the metal bar.

A single pole switch was connected with the wires running to the contact-breaker so that the circuit could be closed by it, and the wheel moved back to a definite starting point without again breaking the circuit and confusing the signals in the reacting-room. A diagram of the connections is given below. The drum used to take the records was turned by hand. The axis of this drum was an endless screw, by means of which it was moved backwards or forwards about 3 mm. at each revolution. The tuning-fork and time-marker were clamped on stands which, after being properly adjusted, were screwed down to a wooden platform. This platform was supported at two corners by metal rods to which it was pivoted. Two metal rings, connected eccentrically by an axis, supported the other two corners. Turning a handle on one end of this axis raised or lowered the platform, thus raising the time-marker and tuning-fork from the smoked drum, or lowering them upon it

[1] Vol. II., p. 422.

till the proper pressure was exerted to secure a continuous record. After some practice the experimenter was able to take about forty records on the drum, which was 14 cm. long and 16.7 cm. in diameter.

The subjects who took part in these experiments were Dr. Pace and several students of the university. Only after a set of experiments was completed were the results made known to the subjects. And though in certain cases the experimenter expected the results obtained, his surmises were not communicated to the subjects beforehand. This, of course, is a very important precaution in psychological experiments, for a biased mental attitude of the subject is very likely to affect the results

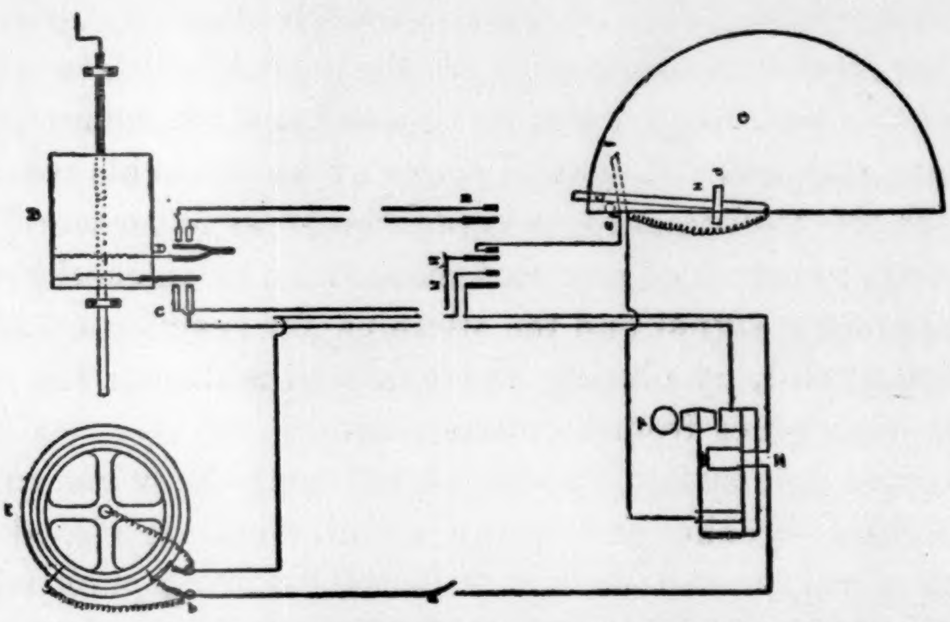


FIG. 1. *A*, time sense apparatus; *a*, bar contact maker; *b*, contact breaker; *c*, one pole switch; *B*, recording drum; *C*, electromagnetic marker; *D*, tuning fork; *E*, electromagnetic sound hammer; *F*, bell; *G*, wooden base for lever; *H*, lever; *d*, arm-rest; *e*, starting past first contact apparatus; *f*, second contact apparatus; *I II III*, batteries.

obtained. Unless otherwise mentioned, the subjects had made several practice-series before the results recorded below were obtained. The principal subjects in these experiments were Dr. Pace (*A*), Mr. McMullen (*B*), Mr. Fagan (*C*), and Mr. Mullaly (*D*). These subjects will hereafter be referred to by the letters in parentheses after their names. Other subjects who made but a few experiments will be referred to by arabic numerals.

V. EXPERIMENTAL RESULTS.

1. Relation between Reaction Time and the Speed of Subsequent Movement when the Subject is not Instructed to make a Rapid Movement.

The few experiments here recorded bear upon the same problem which was undertaken by Professor Delabarre and his associates. Though not the first experiments in the order of time, they are given the first place in this dissertation because they have the nature of a preliminary study rather than an integral part of the problem under discussion. The purpose of this set of experiments was to determine whether or not any relation might exist between the variations in reaction time and the different speeds of movement with which each reaction might happen to be made. While there seemed to be no reason to doubt Professor Delabarre's conclusion, or to expect that the speed of movement would reveal a relation which a study of the force of movement failed to detect, still it was thought best to see if a new method and different apparatus would confirm previous results.

The subjects chosen for these experiments were totally unfamiliar with the work, and knew nothing of the experiments in which the subjects had been told to react with the quickest possible movement. The reason for the choice of such men was to secure a perfectly spontaneous movement. Subjects previously practiced in the quickest possible movement would have found it hard to react with a movement uninfluenced by their previous training. The subjects chosen had never practiced reaction at all; but this was by no means a necessary qualification for the present experiment. For if, by any possibility, a slow movement were the invariable result of a slow reaction and a quick movement of a quick reaction, or *vice versa*, it would not make any difference whether the subject had previously practiced reaction or not. Practice would merely lessen both variations.

The subjects were instructed to respond to the signal for reaction by moving outward the lever above described as soon as they could. Nothing was said to them about making a rapid movement, or taking care to pass the twenty-degree line; but it was found that the natural movement was always greater than twenty degrees. The results are tabulated below. In these and the following tables time is expressed in thousandths of a second (σ) under the tables headed *Reaction*, *Movement*, and *M. V.*, which signify respectively reaction time, movement time, and mean variation. In the column headed *No.* the number of experiments is given. It may be stated here, once for all, that by reaction time I mean the process usually so designated. By movement time, or speed of movement, I mean the time elapsing during a certain part of the movement by which the reaction is executed. The close of the reaction time marks the beginning of the movement time, which is itself completed when the arm passes through the angle agreed upon. Unless otherwise noted the angle in all of the subsequent experiments is one of twenty degrees; the preparatory signal was given two seconds before the tap of the hammer which served as a stimulus for reaction.

SUBJECT 1.

Series I. Angle of Movement 20°.

Reaction.	M. V.	No.	Movement.	M. V.	No.
186	22	10	117	10	10

Series II. Angle of Movement 12°.

Reaction.	M. V.	No.	Movement.	M. V.	No.
158	36	13	140	20	13

SUBJECT 2.

Reaction.	M. V.	No.	Movement.	M. V.	No.
170	33	11	189	23	12

These tables show that, while the variation in movement time under the conditions of these experiments is less than in the reaction time, still it may be considerable. In order to bring out clearly how the two times vary, curves have been plotted which represent the variations in five-hundredths of a second. The continuous lines represent the reaction time, the dotted ones the movement time. The figures along the axis of abscissas

refer to the number of the experiment in each series; those along the axis of ordinates to time in five-hundredths of a sec-

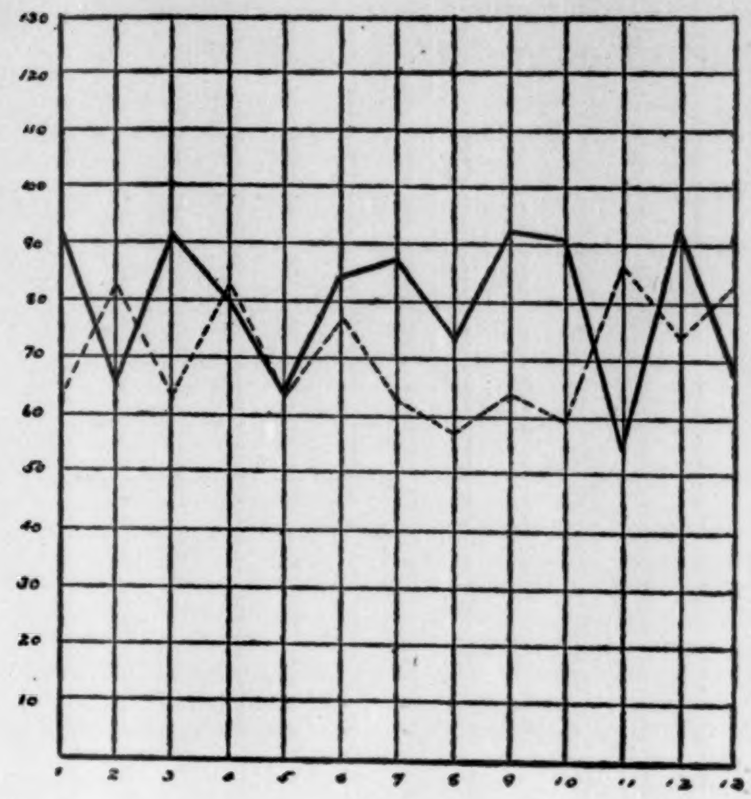


FIG. 2.

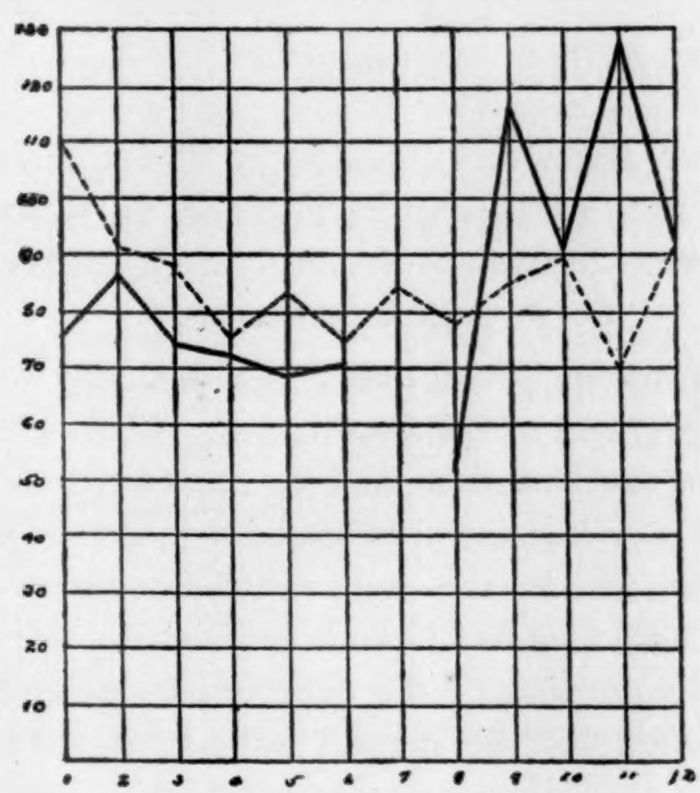


FIG. 3.

ond. Here and elsewhere a break in a curve indicates an unrecorded reaction time or movement time.

From these curves it is evident that the time of reaction may increase concomitantly with the movement time, or the reverse may take place. One may increase or decrease considerably while the other is but slightly changed.

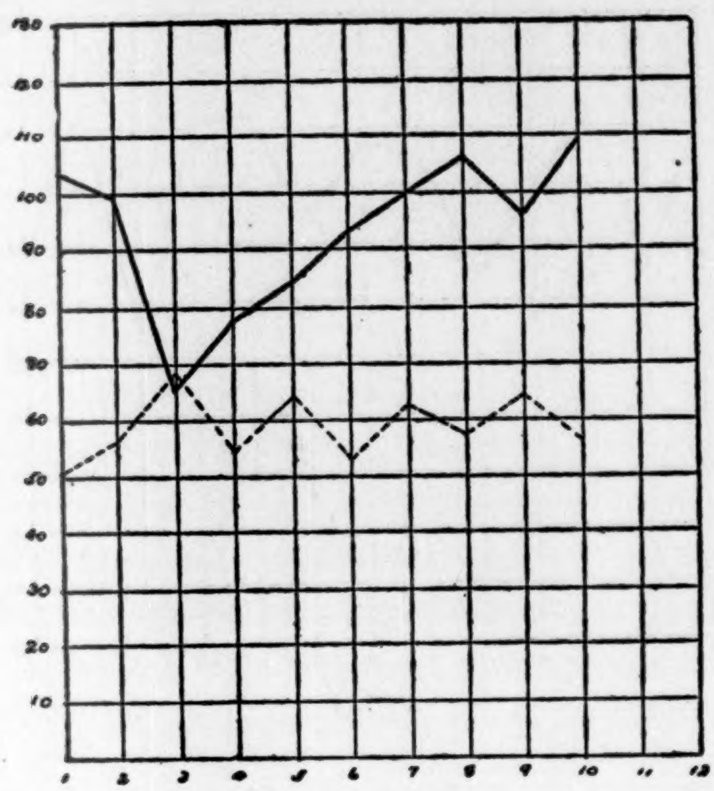


FIG. 4.

2. Relation Between Reaction Time and Movement Time When the Subject is Told to Respond to the Stimulus With the Quickest Possible Movement.

The experiments given under this heading bear upon the fundamental problem of the dissertation. What relation, if any, exists between reaction time and its movement time, when the subject is not only instructed to respond to the stimulus as soon as possible, but also by the most rapid movement of which he is capable? Sufficient has already been said about this problem to bring out the purpose of the experiments given below. But it should be noted that the mental attitude of the subject is not the same as in an ordinary simple reaction. There is another factor present in the preparation for the reaction. Not only is the idea of starting as *soon* as possible in the subject's mind, but there is also an additional motor representation. He must

make the fastest movement of which he is capable. The two ideas, however, do not seem to be antagonistic. They har-

SUBJECT A.

I. Series in which the Preparatory Signal was 4½ Seconds.

Reaction.	M. V.	No.	Movement.	M. V.	No.
167	12	6	118	4	6
165	27	9	116	10	9
155	13	10	115	5	10
137	27	7	113	4	7
122	11	9	120	7	9
156	54	7	136	9	7
137	21	13	103	6	13
144	23	11	121	6	11
155	29	9	122	5	9
143	16	13	101	3	13

monize so well that one does not feel that much more additional effort is required to react with the quickest possible movement

II. Series in which the Preparatory Signal was 2 Seconds.

Reaction.	M. V.	No.	Movement.	M. V.	No.
147	7	10	108	7	11
141	19	10	107	4	10
151	13	8	107	5	10
157	16	10	114	7	10
164	18	13	120	5	14
143	12	14	112	5	14
168	20	12	110	6	13
160	14	12	111	4	12
156	11	11	106	4	12
166	27	9	115	5	9
159	14	11	112	4	10
175	6	10	119	5	12
180	27	10	116	5	10
140	26	8	110	5	11
137	22	11	113	5	14
123	14	10	110	3	10
159	18	10	112	4	11
129	16	9	104	2	10
163	26	11	102	5	13

than if the movement were left undetermined by any previous instruction.

The results obtained from this line of work are here given.

Subject B.

I. Series in which the Preparatory Signal was 4½ Seconds.

Reaction.	M. V.	No.	Movement.	M. V.	No.
109	8	9	100	6	9
109	12	8	100	4	8
117	9	8	99	6	8
147	22	7	92	6	8
136	15	16	102	1	16
142	17	10	101	4	10
143	13	10	98	2	10
152	19	10	96	3	10
133	11	9	92	3	9
148	22	9	100	3	9
166	31	12	94	5	12
133	12	10	96	3	10
161	46	12	91	3	12
136	12	10	95	3	10
141	19	12	94	3	12

II. Series in which the Preparatory Signal was 2 Seconds.

Reaction.	M. V.	No.	Movement.	M. V.	No.
145	16	13	84	6	13
151	18	13	83	4	13
147	14	11	98	5	12
155	11	12	90	4	13
165	11	11	86	3	11
155	15	13	77	3	13
171	14	12	87	3	13
158	39	8	89	3	8
164	30	9	93	3	8
113	26	13	95	4	13
168	22	26	86	5	36
150	10	10	85	6	10
148	35	8	85	4	7
148	11	14	90	2	14
134	12	14	97	2	12
109	8	12	103	3	12
166	32	8	85	4	8
156	11	10	88	6	10
158	20	16	81	4	17
130	14	10	95	3	11
143	16	11	94	4	11
104	9	10	102	1	7
114	10	12	96	3	12
123	7	11	98	4	11
141	16	11	93	3	11
158	16	12	95	5	13

Subject C.

Preparatory Signal 2 Seconds.

Reaction.	M. V.	No.	Movement.	M. V.	No.
103	11	10	102	1	11
134	29	10	97	3	11
145	8	13	104	6	13
151	14	12	102	3	12
156	17	13	117	7	13
142	14	13	107	4	13

Subject D.

I. Series in which the Preparatory Signal was 4½ Seconds.

Reaction.	M. V.	No.	Movement.	M. V.	No.
162	25	11	119	4	12
159	22	10	121	6	12
162	40	13	112	6	13
146	10	10	118	4	11

II. Series in which the Preparatary Signal was 2 Seconds.

Reaction.	M. V.	No.	Movement.	M.V.	No.
191	26	12	97	6	13
170	16	13	93	7	13
148	16	12	104	5	12
164	27	12	113	4	12

In examining the results obtained in these tables the following points are at once evident. The reaction times are usually somewhat longer than muscular reactions made with a telegraph-key to sound stimuli. The mean variations of the reaction times are also longer than those generally found in muscular reactions. The mean variation of the movement time is about five thousandths of a second. Subject *D* had the most variable movement time; but the results recorded under heading (I.) for this subject were the first taken from him; and the series given in the second division were taken at rather long intervals of time. The subject was not trained to the work. Notwithstanding this fact, the time of movement varies but little — less indeed than the time of ordinary muscular reactions.

The fact that the subjects were told to make the quickest possible movement required attention to the movement — the condition most favorable for a muscular reaction. Previous to reaction there was noticed a tension of the muscles, which seemed to show that the strain of attention was directed to the efferent and not to the afferent side of the process. Occasional errone-

ous reactions (*e. g.*, to the preparatory signal) and previous reactions were also noticed. From these facts it seems most probable that the reactions here recorded are what the Wundtian school would consider muscular. The variation from the usual time of muscular reaction and the larger mean variation may be accounted for on two grounds. In the first place, as Cattell has found, the reaction time for the shoulder is longer than that for wrist and finger, which have usually been employed in experiments on reaction time. In the second place, no attempt has been made to secure uniformity by excluding figures which were considerably above the mean value of a series. Only the most exceptionally long (*e. g.*, 400 σ) or short reactions (*e. g.*, 50 σ) have been omitted. And it scarcely need be said that the same rule was applied to the movement time. These very exceptional values, however, occurred but seldom for reaction time, and almost never in the records of the movement. The reason for this method of procedure is, of course, apparent. A relation was sought between the variations in reaction time and those which might exist in the movement time. Hence, only the most exceptional variations in reaction time should be excluded. And it may be appropriate to state here that I cannot wholly agree with Cattell when he says, "Owing, however, to the reflex nature of the reaction, its length is not greatly affected by the condition of the observer, the time of day, the number of reactions already made, nor the amount of practice."[1] I have found quite a difference in the mean value of reaction time on different days; and it would be hard to explain this except on the supposition of variations in the condition of the subject. With subject *C* I am quite certain that anything approaching regularity was only attained after considerable practice. With other subjects I have found that the very first series taken from them was very constant and apparently of the muscular form. The statement that reaction time is not greatly affected by the amount of practice seems to indicate a change in the author's position since 1886. He then wrote: "When a subject has had no practice in making reactions (in which case

[1] 'On Reaction Times and Velocity of the Nervous Impulse,' *Nat. Acad. of Sciences*, Vol. VII., p. 394.

the reaction time is usually longer than 150 σ) I think the will-time precedes the reaction."[1]

Just what position is taken in this paper on the distinction between muscular and sensorial reactions, and the theory of types, will best be mentioned in the theoretical discussion of the results. For the present it will suffice to recognize provisionally Lange's distinction between muscular and sensorial reactions.

The empirical conclusion that the set of experiments warrants is this: When a subject is told to react with the quickest possible movement, the reaction time may be subject to consider-

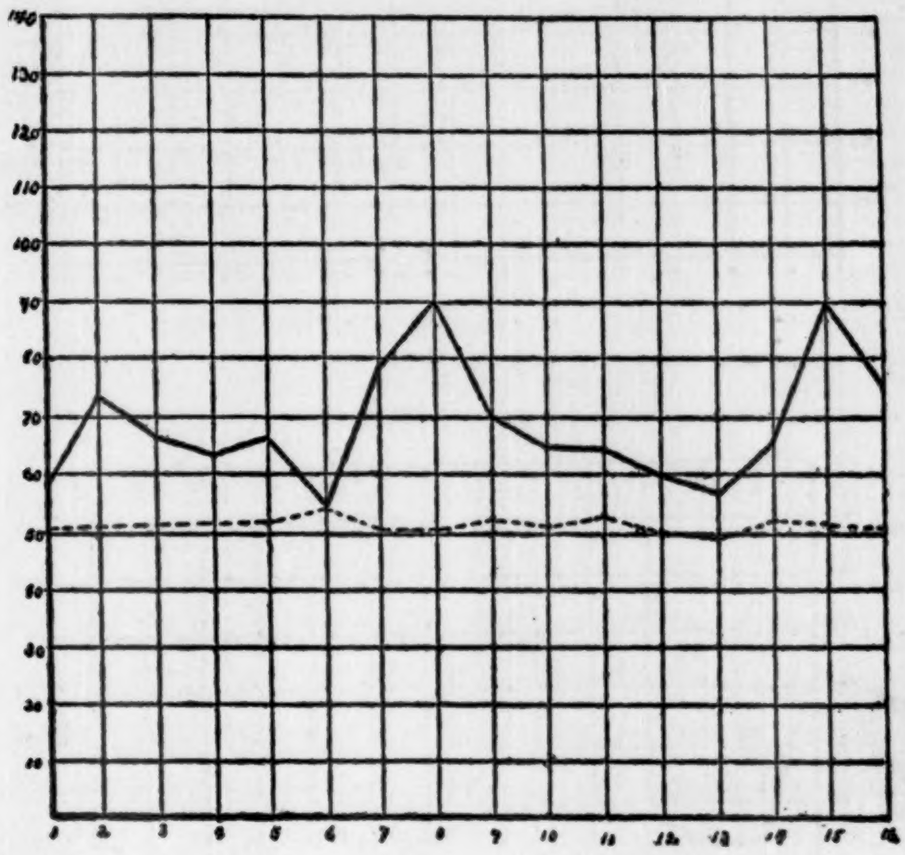


FIG. 5.

able variation, but the movement time remains fairly constant.

To bring out the relation between the individual experiments of the series, curves similar to those given above have been plotted out. The continuous lines represent reaction time, the dotted ones the movement time. The figures along the axis of abscissas refer to the number of the experiment in the series; those along the axis of ordinates to time in five-hundredths of a second.

[1] *Philosophische Studien*, Vol. III., p. 322. Also in *Mind*, 1886, Vol. XI., p. 232.

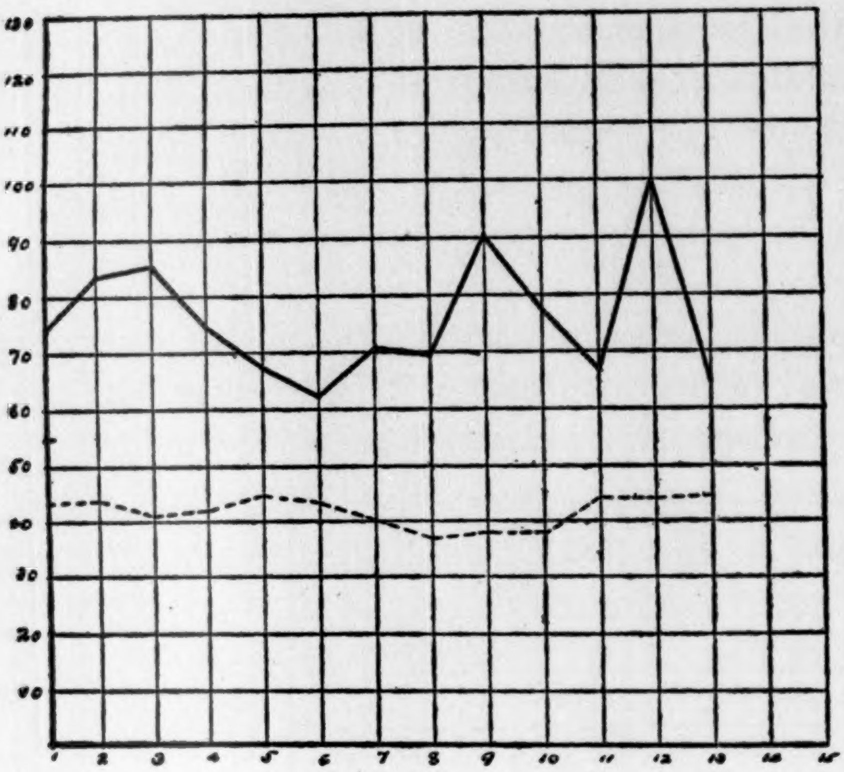
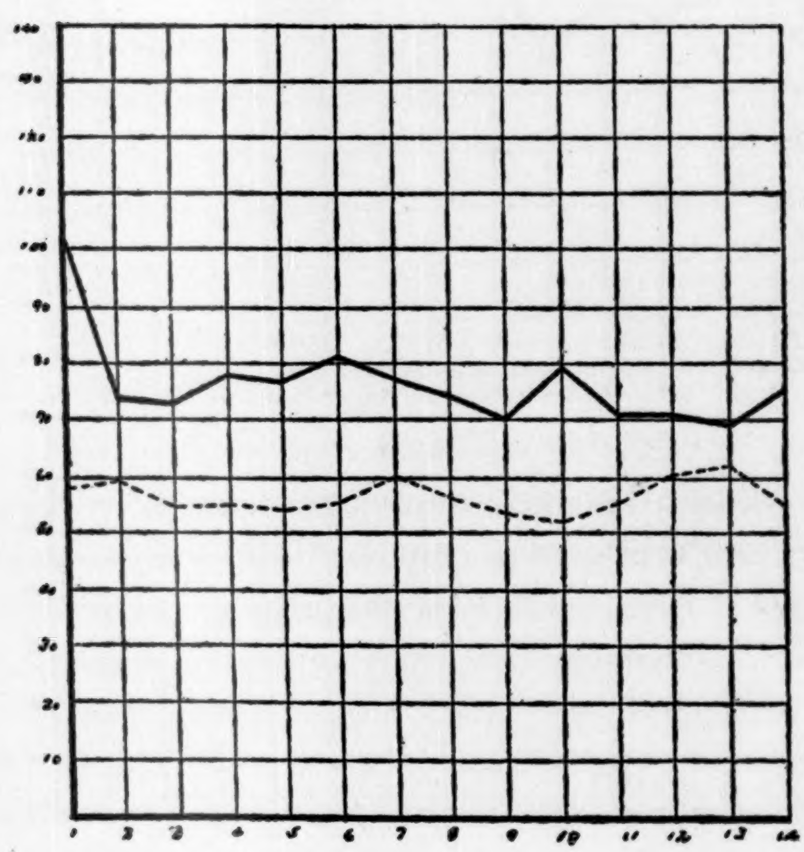

130
120
110
100
90
80
70
60
50
40
30
20
10
1 2 3 4 5 6 7 8 9 10 11 12 13 14 15

FIG. 6.


140
130
120
110
100
90
80
70
60
50
40
30
20
10
1 2 3 4 5 6 7 8 9 10 11 12 13 14

From these curves it is at once evident that the line for reaction is subject to considerably more irregularity than the one for movement. The curves also bring out more than the tabulated results of mean values. For if there were a constant ratio between the slight variations in the movement time and the large differences in the reaction times, it would not appear in the table of mean values. But when we look at these curves it can easily be seen that no such relation exists. A reaction which is longer than the average is sometimes followed by a movement whose value is less than the mean. And then, again, the reverse may take place or both may vary in the same direction. So that it is clearly impossible to state that any direct or inverse ratio exists between the two times.

3. Record of Reaction with Lever, but without Intentional Regard to the Speed of the Subsequent Movement, from a Subject Previously Practiced in Making the Movement as Quick as Possible.

As we said in introducing the last set of experiments, the mental conditions under which they were made varied somewhat from the normal state of preparation for simple reactions. The mental state was more complex. There was an added element in the motor preparation for the reaction. The subject had not only to prepare to move as soon as possible, but also in moving to do so as fast as he could. The added element in the motor preparation for the movement required, as we said, a certain amount of attention to the movement, occasioning thereby a tendency towards muscular reaction. Does this more complex mental attitude in any way affect the time of reaction or not? Will the two ideas so harmonize as to leave the time of reaction unchanged? Or, if the subject is not instructed to make a rapid movement, will this simplification of the motor preparation make it easier for him to react sooner and thereby shorten the reaction? Or will the removal of the idea of making a fast movement make less likely the focusing of the attention on efferent processes, thereby allowing scope for the sensorial form of reaction and lengthening the mean time?

Experiments were made on Subject *A*, in order to throw some light on these questions. Ten series were taken, giving in all a hundred and nineteen experiments. This subject was instructed to react as soon as he could on hearing the proper signal, but to take no care of the subsequent movement whether it was fast or slow. To help towards an indifferent state of mind in regard to the movement the contact apparatus was removed from its position on the wooden base of the apparatus. This subject's previous experiments had been made with an interval of four and a half seconds between the preparatory signal and the stimulus for reaction. The same interval was allowed in the following experiments. The first three series were taken consecutively on December 12, and the rest were made consecutively five days later. No other experiments were made with this subject between these two sets of experiments. Before commencing the first set it had been one day since he had made any experiments at all. These intervals allowed to some extent the previous idea of reacting with a rapid movement to die away.

SUBJECT A.

Reaction.	M. V.	No.	Reaction.	M. V.	No.
144	18	11	129	18	12
141	19	12	155	30	13
161	17	11	131	19	12
139	18	11	133	14	11
140	18	12	132	18	13

In comparing these results with those of the same subject when he attempted to react with the quickest possible movement (preparatory signal four and a half seconds)[1] we find that in general the reaction time in the series just reported is somewhat quicker and the mean variation less than in the former series. At first sight it would seem that these results are against our former statement that attention to making the movement as quickly as possible gave a tendency to react in a muscular manner. But this is not the case, as will appear on a little closer analysis. Subject *A* (Dr. Pace) had practised reaction before taking up this series of experiments. His reactions in those experiments were of the sensory type. When commenc-

[1] *Supra*, p. 21.

ing to act as subject in these experiments his first practice series were generally of the longer form with occasional shorter and apparently muscular reactions. Under the influence, as it would appear, of the idea of making the movement as quick as possible, his reactions soon became quicker until they were of the length already tabulated. They have never been, except in a few series, of the extreme muscular form. When, however, he was told in the series just reported to react without regard for the movement, the influence of his late practice was still felt and the tendency to muscular reaction remained. But, while it seems that direction of the attention towards the attainment of speed in the movement of reaction gives a tendency towards a muscular reaction, still it must be admitted that the more complex mental state may be something of a hindrance — at least with this subject. It is a well-known fact that thinking of how we are to perform an action makes us clumsy. A mental representation of a movement is not the most favorable condition for its execution. But a *mental representation* of a movement and the *muscular tension* in preparation for the movement are two very different things. It is the muscular tension which makes the favorable condition for a muscular reaction, not the mental picture. In reacting, therefore, with the quickest possible movement there are two factors to be taken into consideration. One is the muscular tension which prepares us for a sudden movement and indicates that the attention is strained upon the efferent processes. The other is the mental picture of moving the arm rapidly through the angle agreed upon. The muscular tension gives the tendency towards the quicker form of reaction, the mental picture tends to interfere with a quick reaction. The necessity of making a rapid movement developed this condition of tension, and led subject *A* to drop his previous sensorial form of reaction. The idea in his mind of making a rapid movement tended to interfere with the start of the movement, for when this idea was put out of mind the reaction time quickened and the mean variation was lessened. It is to be regretted that more time was not at hand to go further into the question of practice in relation to our experiments than we have done. The experiments to be reported in the next section bear

more upon the attainment of constancy in the speed of movement than the effect of practice on reaction time.

4. The Effect of Practice on Movement Time.

A full study of the influence of practice on reaction time and movement time should be the object of a special piece of research rather than a part of our present investigation. Several subjects should be practised in the ordinary form of reaction with a telegraph key. They should be practiced in reacting with the lever without care for the speed of the subsequent movement. The effect of directing the attention to stimulus or muscle should be ascertained. And if subjects were found who could react only in the sensorial manner, these especially should be practised in reacting with the quickest possible movement. The results obtained might throw some new light on the discussion between the Wundtian school and the supporters of Professor Baldwin and his theory of types. But such an investigation would be beyond the limits of the present study. The question of practice probably would not have been mentioned at all had it not been noticed that fair constancy in the speed of movement was obtained almost from the very start with all our subjects but one.

To see with what constancy of movement subjects might react who had no practice at all, several students were asked to take part in the experiments who had not previously been employed for this line of work. They were instructed to react as quickly as they could on hearing the proper signal, and move the lever through the angle of twenty degrees as fast as they could. A few unrecorded trials were made to familiarize them with the signals and then the following series were taken.

	Reaction.	M. V.	No.	Movement.	M. V.	No.
Subject (3)	135	20	10	106	3	10
Subject (4)	106	9	11	109	3	11
Subject (5)	151	15	5	127	4	5
Subject (6)	165	31	10	88	3	10

(Subjects (5) and (6) had made a few trial experiments about two months previously.)

With one of these subjects the mean variation in reaction time was nine thousandths of a second, that in movement time

three thousandths. This was the very first series of reactions he had ever made. No doubt further practice would only show that the general statement of Cattell's criticised above[1] holds good for this subject. With all these subjects the constancy in the movement time was attained without previous practice. This is quite likely to be generally true in regard to movement time; but there are a number of normal subjects who do not react constantly except after practice and some never attain to constancy at all.[2]

5. Effect of Varying the Preparatory Signal in Successive Series.

In previous experiments we have been mainly concerned with the relation between reaction time and movement time under what we may consider as normal conditions. If not absolutely normal, they are normal relative to the present research. We have already stated why it was decided to investigate the relation between reaction time and its speed of movement, made as fast as possible. And that relation being the basis of the work, it will serve as a form of comparison for reactions and movements made under the influence of various other conditions. A number of factors have been studied out and are known to influence reaction time to a greater or less extent. It will now be our object to study the influence which these factors may have on both the time of reaction and the speed of movement. The interest which accrues from the study of this problem arises from the fact that some light may be thrown upon the question of just what processes of reaction time these disturbing factors affect. Will those factors which affect reaction time have the same or a different or no effect at all on the movement time?

The study was commenced with those factors which have been supposed to affect reaction time by their influence on the attention. It has long been known that the interval between the preparatory signal and the stimulus for reaction affects the time of reaction.[3] In order to test what effect this interval

[1] *Supra*, p. 24.

[2] *Cf.* E. B. Titchener, *Mind*, 1895, N. S., Vol. IV., p. 506–7.

[3] Dwelshauwers, *Phil. Stud.*, VI., p. 217–249. Martius, *Phil. Stud.*, VI., p. 199, ff.

might have on the movement, several series were taken consecutively in which that interval was of different lengths. The results are given below. The dates are published for each series, and when several series have the same date, they were taken consecutively. The reason for taking these consecutive series was that both the time of movement and the reaction time vary somewhat from day to day. This variation must be eliminated as far as possible if any true comparison is to be made.

SUBJECT A.

I. Preparatory Signal One Second.

	Reaction.	M. V.	No.	Movement.	M. V.	No.
12/25	141	12	12	114	6	12
12/25	157	10	10	107	4	10
12/25	153	15	10	105	2	12
12/25	169	15	9	108	5	12
12/25	155	23	13	107	5	14
Serial Means,	155	15		108	4	

II. Preparatory Signal Two Seconds.

	Reaction.	M. V.	No.	Movement.	M. V.	No.
12/25	147	7	10	108	7	11
12/25	141	19	10	107	4	10
12/25	151	13	8	107	5	10
12/25	157	16	10	114	7	10
Serial Means,	149	14		109	6	

SUBJECT C.

I. Preparatory Signal One Second.

	Reaction.	M. V.	No.	Movement.	M. V.	No.
12/26	150	12	12	105	4	12
12/26	171	19	12	99	3	12
12/26	118	24	10	101	4	13
Serial Means,	146	18		102	4	

II. Preparatory Signal Two Seconds.

	Reaction.	M. V.	No.	Movement.	M. V.	No.
12/26	103	11	10	102	1	11
12/26	134	29	10	97	3	11
12/29	144	8	13	104	6	13
12/29	151	14	12	102	3	12
Serial Means,	133	16		101	3	

III. Preparatory Signal Three Seconds.

	Reaction.	M. V.	No.	Movement.	M. V.	No.
12/26	152	17	9	106	5	11
12/26	157	20	10	97	3	12
12/29	144	8	13	100	3	13
12/29	140	10	11	99	4	13
Serial Means,	148	14		101	4	

These results indicate that when the preparatory signal comes two seconds before the reaction stimulus, the conditions are slightly more favorable for reaction than when it comes at either one or three seconds before. But a preparatory signal two seconds beforehand does not seem to be any more favorable for the execution of the movement than when it comes at one or three seconds before the stimulus for reaction.

6. Series Taken With an Irregular or no Preparatory Signal.

The difficulty under which the attention labors in preparing for a reaction when the preparatory signal comes at one or three seconds beforehand, is certainly slight if compared with the strain of keeping in readiness for reaction when the warning signal is given at irregular intervals or not at all. At the end of such a series of experiments there is a feeling of relief which is indicative of the strain under which the attention was laboring. Previous investigators have found that the efforts of a subject to keep his attention focused were not successful and that the time of reaction was considerably lengthened. But will this condition of mental strain have the same effect on the movement time? In considering this problem we must remember that we are dealing not only with a disturbance of the attention, but also with a stimulus arising from the mental effort to keep the attention focused. The disturbance we may consider as negative, for the conditions are merely unfavorable to concentration of the attention. Nothing positive is done to distract the subject. He endeavors to make up for the lack of a warning signal by increased efforts to focus his attention. What will be the effect of this mental activity on the movement time?

Subject A.

Series I. Preparatory Signal 4½ Seconds.

Reaction.	M. V.	No.	Movement.	M. V.	No.
167	25	9	114	1	7

Series II. No Preparatory Signal.

Reaction.	M. V.	No.	Movement.	M. V.	No.
286	76	7	107	4	7

SUBJECT B.

Series I. Preparatory Signal 4½ Seconds.

Reaction.	M. V.	No.	Movement.	M. V.	No.
141	19	12	94	3	12

Series II. Preparatory Signal None.

Reaction.	M. V.	No.	Movement.	M. V.	No.
210	20	11	88	2	11

Series I. Preparatory Signal None.

Reaction.	M. V.	No.	Movement.	M. V.	No.
172	12	26	91	3	27

Series II. Preparatory Signal 2 Seconds.

Reaction.	M. V.	No.	Movement.	M. V.	No
148	11	14	90	2	14

Series I. Preparatory Signal 2 Seconds.

Reaction.	M. V.	No.	Movement.	M. V.	No.
134	12	14	97	2	12

Series II. Preparatory Signal None.

Reaction.	M. V.	No.	Movement.	M. V.	No.
178	20	25	95	5	23

Series I. Preparatory Signal None.

Reaction.	M. V.	No.	Movement.	M. V.	No.
196	41	19	75	4	21

Series II. Preparatory Signal 2 Seconds.

Reaction.	M. V.	No.	Movement.	M. V.	No.
158	20	16	80	4	17

Series III. Preparatory Signal None.

Reaction.	M. V.	No.	Movement.	M. V.	No.
209	29	40	79	12	41

SUBJECT D.

I. Preparatory Signal 4½ Seconds.

	Reaction.	M. V.	No.	Movement.	M. V.	No.
	162	40	13	112	6	13
	146	10	11	118	4	11
Serial Means,	154	25		115	5	

II. Preparatory Signal Irregular, 1 Second to 1 Minute.

	Reaction.	M. V.	No.	Movement.	M.V.	No.
	278	50	10	114	4	10
	232	38	9	110	6	9
	272	54	8	113	3	8
Serial Means,	261	47		112	4	

In the results just tabulated we notice an evident lengthening of reaction time when no preparatory signal is given. The mean variation for the reaction time without preparatory signal was generally larger than that of the normal series. These

results do not altogether agree with the position taken by Wundt. Quoting the work of Dwelshauwers, he says: "When the stimuli are given without preparatory signal, the greater or less endeavor to strain the attention has its effect indeed upon the length, not upon the regularity of the reaction." [1]

As to the movement time, it is clear that it is not affected to such an extent as the reaction time. And, furthermore, whatever effect there may be upon it is not in the same direction. For in the series without preparatory signals the movement times were somewhat quicker than in the normal series. This quickening, however, was slight — less than one hundredth of a second. And though it could scarcely be considered to prove conclusively that the strain of attention acted as mental stimulus which reinforced the movement, still it would be hard to account for the acceleration of the movement time on any other ground.

7. Series Taken without Any Preparatory Signal and While the Subject Was Adding.

The disturbance of the attention occasioned by omitting the preparatory signal is, as we have seen, negative in its character. It merely increases the difficulty of attending, without doing anything positive to disturb the attention. It remains to be seen what will be the effect of joining to the negative disturbance one of a positive character. Will this double disturbance lengthen the movement time, or interfere with its regularity? In order that the results may be more closely comparable with the last, it will be best to have the disturbance of an intellectual rather than a sensory kind. For, as we have seen, the increased effort of the subject to strain his attention may have acted as a mental stimulus to the movement. Is it true, as M. Féré thinks,[2] that mental activity will increase the immediate output of muscular energy?

But here we are concerned with the effect of disturbances of the attention on reaction time and movement time, rather than

[1] 'Grundzüge der physiol. Psychol.,' 4th ed., Vol. II., p. 349.

[2] 'Sensation et Mouvement,' Paris, 1887, p. 7. 'Note sur l'influence reciproque du travail physique et intellectuel,' *Journal de l'Anatomie et de la Physiologie*, 1901, Vol. XXXVII, p. 625, ff.

the reinforcement of the movement by various stimuli. This latter problem will be given special attention further on. But the two questions have points in common, and in investigating one it becomes necessary to consider the other.

A disturbance of the attention, which at the same time is of an intellectual character, can be produced when the subject is told to carry on a process of addition during a series of experiments. Subject *A* was told to add seventeen to seventeen from the beginning of the series of experiments to the end, and to try at the same time to be ready to react at the tap of the hammer. When asked afterwards what influence the process of addition seemed to have, he replied that the attempt to add did not seem to increase greatly the difficulty of keeping himself in readiness to react. Subject *B*, however, found it very difficult and disagreeable.

In the results reported below, consecutive series are grouped together. In each group there is a normal series which is to be compared with those affected by the departure from normal conditions.

In all series the preparatory signal was given two seconds before the tap of the hammer, unless otherwise noted. This remark about the normal series applies not only to this section, but also to all subsequent sets of experiments.

The series are tabulated in the order in which they were taken.

SUBJECT A.

		Reaction.	M. V.	No.	Movement.	M. V.	No.
I.		268	32	12	115	7	12
	(Normal),	175	6	10	119	5	12
II.	(Normal),	148	20	12	126	6	12
		248	20	11	114	5	11
III.		317	50	10	119	5	10
	(Normal),	180	27	10	116	5	10
IV.	(Normal),	197	17	15	106	3	15
		268	24	23	111	2	25
V.	(Normal),	164	16	13	114	4	13
		254	27	24	117	3	21

SUBJECT B.

		Reaction.	M. V.	No.	Movement.	M. V.	No.
I.	(Normal),	168	22	26	86	5	36
		341	42	26	93	9	27
	(Normal),	150	10	10	85	6	10

		366	54	10	106	11	9
II.	(Normal),	148	35	8	85	4	7
		357	80	13	113	15	13
III.		298	65	10	98	4	14
	(Normal),	166	32	8	85	4	8
		265	35	10	104	8	16
IV.		279	23	10	102	10	10
	(Normal),	156	11	10	88	6	10
		282	43	13	105	12	13

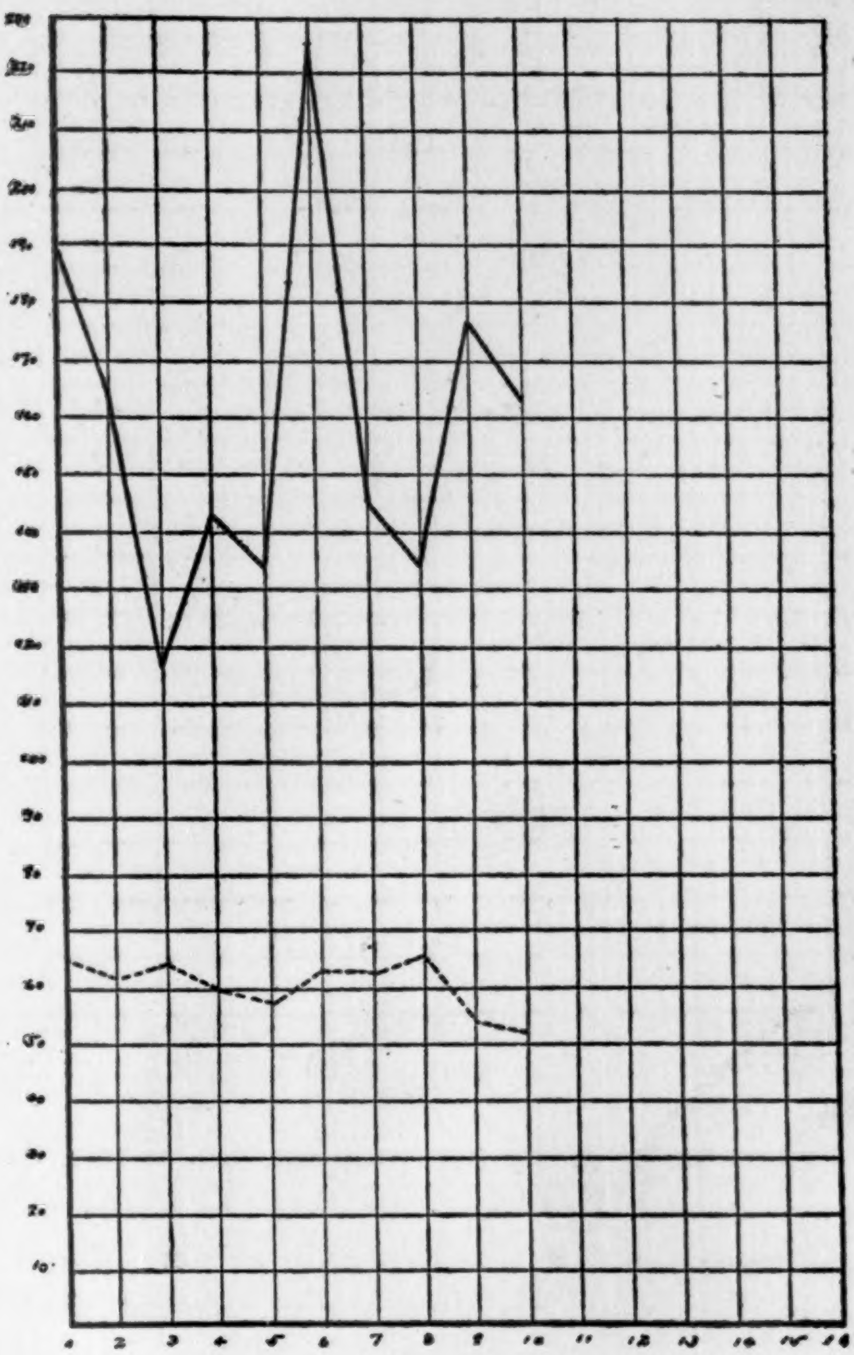


FIG. 8.

From these results it is perfectly clear that subject *B* cannot carry on a process of addition and react with as quick a

movement as he is capable of making under normal conditions. But it is to be remembered that this subject found the process of addition a disagreeable as well as a difficult task. With subject *A*, who found the process but a slight source of distraction, the lengthening of the movement time is not so great nor is it constant. On two occasions his movement was quicker while adding than under normal conditions.

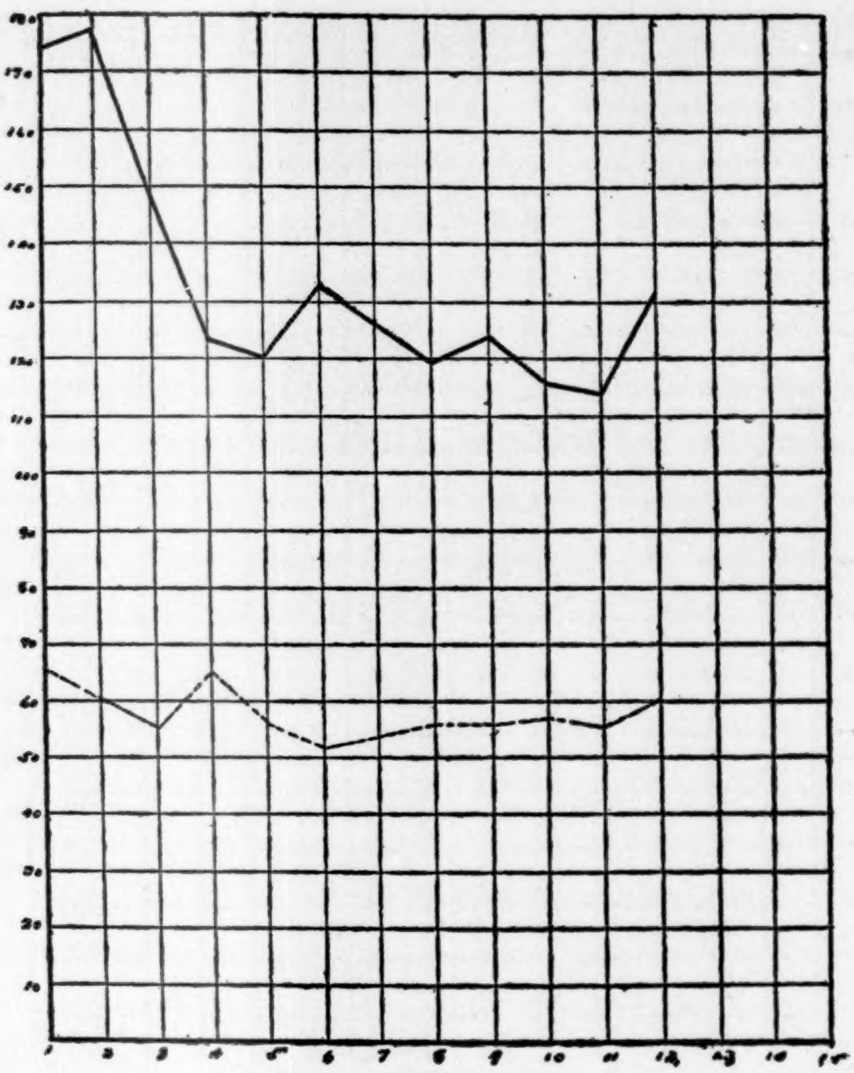


FIG. 9.

In the experiments in the previous section, where the conditions were not favorable for concentrating the attention upon the reaction and movement, it was certainly evident that the reaction time was lengthened and that the movement time was not retarded, but if anything shortened. From this we can conclude that while the attention — owing to the lack of a preparatory signal — wandered in these experiments, still the subjects were capable of maintaining the state of preparation for their maxi-

mum motor discharge in spite of the fluctuations of the attention. The experiments in this section show that with subject *B* the disturbance of the attention was certainly greater than was compatible with maintaining the state of preparation for the maximum motor discharge. With subject *A* this was generally but not always the case. The point to be noted at present — but which will be brought out more fully later — is that the state of preparation for the maximum motor discharge can be maintained in the midst of fluctuations of the attention more or less extensive.

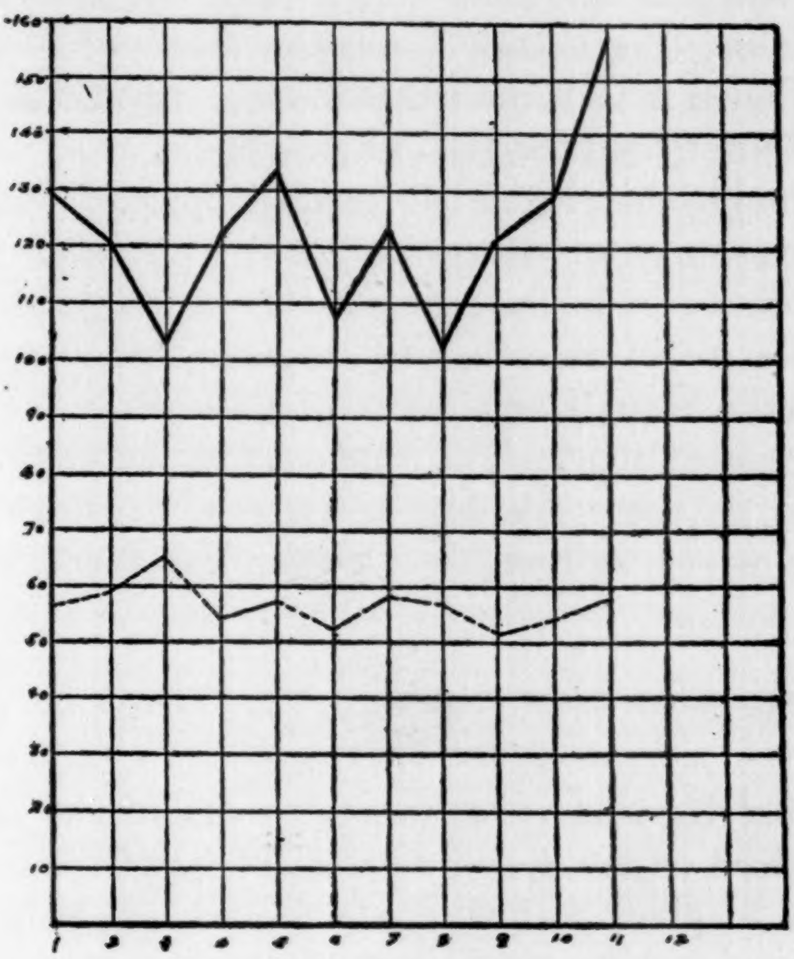


FIG. 10.

Besides the interpretation of the mean values of the experiments, it may be of interest to inquire into the variations of the individual experiments. It has been pointed out by Wundt[1] that when a very loud signal is given for reaction it sometimes seems to be of unexpected intensity, so that the subject is startled and the reaction time lengthened. Though under nor-

[1] 'Grundzüge der physiologischen Psychologie,' 5th ed., III., p. 430.

mal conditions we found no variation in the movement time concomitant with that in the reaction time, some such variation might be expected in series made under disturbing influences. Perhaps the shock of surprise might lengthen or shorten the movement time. At any rate, if the same set of conditions govern both the reaction time and the movement time we should expect to find that one would vary directly or inversely as the other. To bring out the relation between the individual experiments of the series, curves have been plotted out to represent the variations within each series. The continuous lines are the curves of reaction time; the dotted ones, of movement time. Numerals along the axis of abscisscas refer to the number of each experiment in its series: those along the axis of ordinates refer to time in five-hundredths of a second.

In examining the plotted curves it will appear that the longer reaction times are not always followed by shorter movement times. An increase in the reaction may be followed by either a decrease or an increase in the movement time. And reactions above the mean have movements above or below the mean of the movement time. The supposition, therefore, that the longer reaction times are due to a set of causes which also govern the movement time, finds no confirmation in the plotted curves.

8. Compound Reaction and the Speed of Subsequent Movement.

After studying some of the disturbances of simple reaction and their effect on the speed of the subsequent movement, it seems to be in place to investigate the relation between compound reaction and its movement time. Our previous experiments have indicated that increase of mental activity augmented the motor discharge of reaction. If, now, we make it necessary for the subject to perform one or more psychic acts between the signal for reaction and the execution of the movement, what relation will exist between the time of reaction and of movement? Will the movement time maintain its normal constancy? Will the interpolated process reinforce or tend to inhibit the subsequent movement?

The compound reaction selected for this test was that of choice between a movement and no movement. This kind of reaction is indeed simpler than that of choice between two movements. But the more complex form has not always been found to be the longer one.[1] In this form of reaction it is necessary for the subject to perceive the difference between two signals, and choose to react or not. If he chooses to react, these processes of distinction and choice must intervene between the time of hearing the signal and the muscular response.

In order to make this series of experiments, a second signal was arranged which gave a tap differing in sound from the previous stimulus for reaction. The new sound was made by letting the armature of an electromagnet fall about five millimeters on a piece of sheet-iron, forty-five centimeters square, supported on four brass legs about fifteen millimeters high. This piece of sheet-iron was used in other experiments for giving a very loud signal. The tap made by the fall of the armature was not, however, loud, but perfectly distinct. The electromagnet was worked by an independent circuit. To break this current a contact-breaker could be substituted at will for the metal bar on the time-sense apparatus above described. The signal not to react was produced by the new arrangement. The signal to react was given as in the previous experiments.

SUBJECT A.

		Reaction.	M. V.	No.	Movement.	M. V.	No.
I.		296	58	8	98	3	9
	(Normal),	156	11	11	106	4	12
II.		278	29	9	104	3	13
	(Normal),	166	26	9	115	5	9
III.		215	43	8	113	7	9
	(Normal),	159	14	11	112	14	10
		212	55	12	118	6	12

SUBJECT B.

		Reaction.	M. V.	No.	Movement.	M. V.	No.
I.		294	60	9	104	8	12
	(Normal),	155	11	12	90	4	13
II.		297	51	9	95	11	7
	(Normal),	165	11	11	86	3	11
III.		381	71	10	85	4	11
	(Normal),	155	15	13	77	3	13
IV.		319	66	10	96	6	10
		328	76	10	99	6	10
	(Normal),	171	28	12	87	3	13

[1] Cf. Wundt, 'Grundzüge der physiologischen Psychologie,' 5th ed., III., p. 461.

In examining these tables it will be found that for Subject *B* the mean value of the movement time was always several thousandths of a second longer when it comes as the result of a compound reaction. The corresponding mean variation is also lengthened. For Subject *A* this result occurs only in the last group of series reported, and then the lengthening is but slight.

I. SERIES IN WHICH SUBJECT B WAS DIRECTED TO GIVE THE GREATER SHARE OF HIS ATTENTION TOWARDS REACTING WITH A QUICK MOVEMENT.

		Reaction.	M. V.	No.	Movement.	M. V.	No.
I.	(Normal),	109	8	12	103	3	12
II.		220	24	22	98	2	24
I.		199	40	26	93	2	26
II.	(Normal),	130	14	10	95	3	11
I.	(Normal),	143	16	11	94	4	11
II.		195	60	19	95	3	20
I.	(Normal),	105	9	10	102	1	9
II.		185	49	25	101	3	26

(From four to seven erroneous reactions occurred in each series of compound reactions.)

II. SERIES IN WHICH SUBJECT B WAS INSTRUCTED TO TAKE CARE TO REACT ONLY TO THE RIGHT SIGNAL; BUT AT THE SAME TIME TO MAKE THE QUICKEST POSSIBLE MOVEMENT.

		Reaction.	M. V.	No.	Movement.	M. V.	No.
I.	(Normal),	114	10	12	96	3	12
II.		227	40	29	96	2	29
I.		269	48	25	92	1	25
II.	(Normal),	123	7	11	98	4	11
I.	(Normal),	141	16	11	93	3	11
II.		256	26	10	92	2	10
I.		219	36	28	93	2	28
II.	(Normal),	158	16	12	95	13	5

(In the first part of three of these series of compound reactions the subject made one erroneous reaction, but was immediately warned. After the warning each series was completed without another error.)

The compound reaction time for Subject *B* was longer than that for Subject *A*. This suggested the supposition that perhaps Subject *B* paid such attention to waiting for the signals that he did not exert his maximum effort to make the quickest possible movement. Accordingly, it was decided (1) to take several series for this subject in which he was directed to pay more attention towards reacting with a quick movement than taking care to react only to the proper signal, and then (2) to direct him to

take care not to make any mistakes, but at the same time to react with the quickest possible movement. The results are given above.

We have here results obtained from two methods of directing the attention in a compound reaction. If the attention is directed towards making a rapid movement the reaction time is quicker than when the subject directs his attention to the signal, *i. e.*, takes care to make no mistakes. The compound reactions made under these conditions appear to have some analogy to the muscular and sensorial simple reactions. But when the subject is told to give the greater share of his attention towards making a quick movement there is scarcely a full compound reaction. The process of choice is perhaps eliminated and we have in the greater part of the series a full sensorial reaction. But when the subject is told to take care tha the make no mistakes we have a true compound reaction with a complete process of choice interpolated.

As to the movement time there is but little difference in the two sets of experiments. In both the time of movement is usually a little quicker for compound reactions than for those made under normal conditions. It seems that the slight increase of mental activity demanded for the compound reaction acted as a stimulus which slightly reinforced the motor discharge. This result agrees with those obtained with Subject *A*, but is a reversal of the results first obtained from Subject *B*. The compound reaction times in Subject *B*'s last two sets of experiments are shorter than in his former. The most probable explanation of these facts is that in his first set of experiments Subject *B*'s attention was so exclusively turned towards the signal for reaction that he did not take the proper care to make the movement as quick as possible. If this be true, then we have in the first set of experiments from Subject *B* something like a sensorial compound reaction and in the second set a muscular compound reaction.

9. Relation Between the Movement of Reaction and an Independent Voluntary Movement.

All the movements measured in the preceding series of experiments were the immediate result of responding to the

stimulus for reaction. The chain of connection between the ear and the muscles concerned in the movement was in some manner prepared by a voluntary act which preceded the stimulus for reacting. In those reactions that were muscular it will generally be admitted that no new act of the will intervened between the stimulus for reaction and the movement. In the sensorial reactions (*e. g.*, those made without any preparatory signal) there was at least some preparation for the movement, by a previous act of the will, before the stimulus was given. Perhaps there might be some difference in the time of movement when it arises as the immediate result of a voluntary act and not in response to a stimulus. To test this point the subject was told that, on hearing the signal, he should wait a short time and then, after resolving to make the quickest movement he could, execute it to the best of his ability. In this way a movement was secured which came as the immediate result of a voluntary act. Consecutive series were taken of the voluntary movement and of the reaction and its movement. As usual we have grouped the consecutive series together in reporting the results below.

SUBJECT A.

		Reaction.	M. V.	No.	Movement.	M. V.	No.
I.					115	8	24
	(Normal),	137	23	11	113	5	14
	(Normal),	145	14	10	113	4	10
					106	6	22
	(Normal),	123	14	10	117	5	14
					117	5	14
					118	10	13
II.					114	8	39
	(Normal),	159	18	10	112	4	11
					116	4	25
III.					107	5	24
	(Normal),	163	26	11	102	5	13

SUBJECT B.

		Reaction.	M. V.	No.	Movement.	M. V.	No.
I. Prep. Signal 4½ s.							
	(Normal),	117	9	8	100	6	8
					110	8	8
II. Prep. Signal 4½ s.							
	(Normal),	133	12	10	96	3	10
					88	3	16

III. Prep. Signal 4½ s.						
				89	1	6
(Normal),	161	46	12	91	3	12
				93	0.5	4
IV. Prep. Signal 4½ s.						
				90	3	10
	136	12	10	95	3	10
				93	3	10
V. Prep. Signal 2 s.						
				92	5	27
	113	26	13	95	4	13
				95	4	16

When we examine these results it seems perfectly clear that with Subject *B* the speed of what may be called the purely voluntary movement cannot be said to be constantly quicker or slower than that made in response to the signal to react. The variations lean now a little to one side and now to another. The mean deviation from the normal (without regard to plus or minus signs, of course) is four thousandths of a second. With Subject *A*, however, it must be noted that the normal was quicker by several thousandths of a second, except in one case, when the voluntary movement was seven thousandths of a second quicker than the preceding normal, and four thousandths of a second quicker than the succeeding normal. The mean deviation from the normal for this subject is five thousandths of a second. It may be noted that with Subject *B* the tendency towards a muscular reaction is stronger than with Subject *A*.

The conclusion that we are justified in drawing from these results is that no decided difference exists between the time of a movement made in reaction and that made by a purely voluntary act.

10. The Effect of Sensory Stimuli on the Time of Reaction and of Movement.

Before presenting the results obtained in this part of the work, it will be well to give some account of the literature which bears upon the effect of various stimuli on voluntary movement.

One of the earliest and most extensive workers in this line is M. Charles Féré. The first edition of his 'Sensation et Mouvement' was published in 1887. In that work he put forward a number of conclusions at which he had arrived by means of his

experiments with the dynamometer. He there published the opinion that 'momentary exercise of the mind provokes a momentary exaggeration of voluntary movements.'[1] He also found that the exercise of any group of muscles other than those used in working the dynamometer increased the force of the hand a sixth or a fifth part or even more. Speaking produced a like effect. He also studied the influence of the suggestion of a movement on subjects abnormally affected by excitatory or depressive agents. Such a subject could be made to feel a movement in his own hand and finally to execute irresistible, rhythmic flexions, merely by watching the movements of the experimenter. If, however, at the point where the subject commenced to feel the sensation of movement, his hand were placed in the dynamometer, the force of movement then registered would be a third or a half greater than the normal pressure. M. Féré concluded from these and certain other experiments that 'the energy of a movement is in relation with the intensity of its mental representation.'[2] He also found that the shorter the wave-length and the greater the amplitude of wave-length in a sound, the greater was its power to reinforce movement.[3] Similar results were found to hold good for light stimuli.[4]

M. Féré continued this line of work by subsequent researches with Mosso's ergograph. One of these pieces of work, which deals with the effect of a number of sensory stimuli, was published in 1901, in the *Journal de l'Anatomie et de la Physiologie*.[5] The method of procedure in the experiments there reported was:

1. The reaction times of two index fingers and of the left middle finger were taken.

2. Two records of pressure were taken from each hand on the dynamometer.

3. The middle finger of the left hand was placed in the dynamometer and a weight of three kilograms was lifted every second. It is not stated that each series was limited by the exhaustion of the middle finger.

[1] P. 7.
[2] *Op. cit.*, p. 14.
[3] *Op. cit.*, p. 34, ff.
[4] *Op. cit.*, p. 41, ff.
[5] Vol. XXXVII., pp. 1–79.

4. A rest of three minutes was allowed, during which each hand was again tested on the dynamometer. A new series was then begun and this method of procedure continued through a number of series, varying from seven to sixty.

5. At the end of the whole set the pressure of each hand was again taken, and the reaction time of the two index and the left middle fingers were again taken.

The results indicated an oscillation in fatigue throughout the various sets of experiments. The pressure of the left hand steadily diminished; but at the end of four or five series that of the right hand was often augmented. At the end of the experiment the reaction time of the two index fingers was found to be less than at the beginning; but that of the middle finger was increased. It was also found that moving the legs and counting in a loud voice reinforced the working-finger. More work could be done with the eyes open than closed, and more under the influence of red light than any other. Sounds both harmonious and discordant increased the power of working. Musk and ethereal odors, the taste of sugar, acetic acid and sulphate of quinine all reinforced the working finger. Alcohol and bouillon merely taken into the mouth increased the amount of work that could be done more than when swallowed. Tastes were found, as a rule, to reinforce the working finger more than odors, and both together operated more powerfully than either separately. Cutaneous sensations, such as heat, cold, and rubbing, had also a tonic effect.

In another article in the same volume of this magazine, M. Féré came to these conclusions: "Le travail mécanique de la main gauche est moins influencé par le travail intellectuel que le travail mécanique de la main droite. Le travail de la main gauche gagne moins quand il coïncide avec un travail intellectuel facile, il perde moins quand il coïncide avec un travail intellectuel relativement compliqué."

M. Féré has made a number of other studies in the reinforcement of voluntary muscular contractions; but it will not be necessary to summarize them here. A number of them will find mention in the bibliography. Those already reported show the general trend of his line of work.

Ludwig Hofbauer[1] found that loud raps, the slamming of a door or such sound stimuli, were too weak to make any manifest change in an ergogram. Consequently he tried the sound made by firing a revolver. The contraction recorded after such a loud report usually jutted far above those before or after it. On closer examination it was found that when the report came less than .4 of a second before the ordinary signal for contraction, the contraction was nearly always reinforced. But if a longer time intervened the contraction was somewhat lessened. When the report came from .27 to .49 of a second after the ordinary signal the contraction was reinforced. From these and other experiments made with visual and cutaneous stimuli he came to the conclusion that if the excessive stimulus comes when the attention is focused upon a movement about to be performed it reinforces the movement. But if the excessive stimulus comes before the will is prepared to act then the voluntary movement is inhibited.

Mr. Allen Cleghorn repeated the same work, confirming the results of Hofbauer, and also called attention to the fact that the relaxation following such a reinforced contraction is quicker and more complete than when no stimulus is given.[2]

Most of the literature on the reinforcement of voluntary muscular contractions is, as a rule, professedly concerned with fatigue rather than problems of reinforcement and inhibition. And, indeed, in all work with the ergograph, such as that of M. Féré, it may be questioned whether or not the sensorial stimulus directly reinforces the movement, or whether it does so indirectly by tending to eliminate one of the several factors which help to bring about fatigue. The effect of alcohol on reaction time and ergographic work has been the subject of much experimental research. Its working is, no doubt, of a much more complicated character than that of a mere sensory stimulus.

It is certainly desirable to study the reinforcement of voluntary muscular contractions under simpler conditions than those

[1] 'Interferenz zwischen verschiedenen Impulsen im Centralnervensystem,' *Pflüger's Archiv für die ges. Physiologie*, Vol. LXVIII., pp. 546–559.

[2] *American Journal of Physiology*, 1898, Vol. I., pp. 336–345.

offered by the ergograph. We cannot be sure that stimuli which strengthen muscular contractions under the influence of an ever-increasing fatigue will have the same effect on single voluntary contractions when the element of fatigue is practically eliminated. The apparatus already used in these experiments affords the opportunity of studying the effects of sensory stimuli on single voluntary contractions. Not only can we study the single contractions, but also the reaction of which they are the result. And an interesting problem at once presents itself: will sensory stimuli which quicken or lengthen reaction time have the same effect on the time of the subsequent movement?

The stimuli which have been studied are:

(*a*) A continuous noise made by the interrupter of an induction coil.

(*b*) The noise of this induction coil plus a slight shock received by holding the electrodes in the hand. This shock was not painful.

(*c*) An intermittent sound made by a metronome beating sounds.

(*d*) A very loud signal for reaction.

To give this loud signal, some additions to the apparatus already employed were required. A hammer was pivoted so that it could fall through an angle of sixty degrees on the plate of sheet-iron mentioned above.[1] In one corner of this plate a spring made of phosphor-bronze was fastened to an insulated support. The other end could be held up from the plate of sheet-iron on an insulated latch. When the hammer fell, it drove the spring down upon the plate of sheet-iron, giving at the same time, as a signal for reaction, a single loud report. An electric contact was thereby established through wires running from the metal plate and the spring. This contrivance was substituted for the electromagnetic sound-hammer in the circuit above described.[2] The hammer which fell on the metal plate could be held in an almost upright position by means of an electromagnet. Breaking this circuit let the hammer fall until it hit the spring. The plate of sheet-iron rested on a wooden

[1] P. 41.

[2] P. 14 ff.

box, which served as a sounding-board. Its level being somewhat above that of the hammer's pivot, the extent of the swing was limited to about sixty degrees.

It was found necessary to introduce this spring for making an electric contact with the plate, rather than to have it established directly through the hammer. The first experiments were made by the latter method; but the rebound of the hammer falling through so great a distance falsified the results of the reaction time. These early series, however, are published below, the reaction time for the loud signal being omitted. The time of movement, however, could not be effected by the rebound of the hammer, for the contact must be made by the time the subject commences to react, or no record at all is obtained.

The results are divided into sections. The effect of the sound of the induction coil and the shock were studied together and form a single section.

Section A. The effect of a continuous sound and of a continuous electric shock on reaction time and movement time.

SUBJECT A.

Series I. Disturbance Caused by the Sound of an Induction Coil.

Reaction.	M. V.	No.	Movement.	M. V.	No.
202	36	9	125	4	10

Series II. Normal.

Reaction.	M. V.	No.	Movement.	M. V.	No.
164	18	13	120	5	14

Series III. Disturbance Caused by the Sound of an Induction Coil and a Continuous Shock.

Reaction.	M. V.	No.	Movement.	M. V.	No.
177	23	12	127	11	13

Series IV. Same as Series I.

Reaction.	M. V.	No.	Movement.	M. V.	No.
162	11	13	116	6	13

Series V. Normal.

Reaction.	M. V.	No.	Movement.	M. V.	No.
146	17	14	112	5	14

Series VI. Same as Series III.

Reaction.	M. V.	No.	Movement.	M. V.	No.
166	18	10	119	4	11

SUBJECT A.

Series I. Normal.

Reaction.	M. V.	No.	Movement.	M. V.	No.
168[1]	20	12	110	6	13

Series II. Disturbance Caused by the Sound of an Induction Coil.

Reaction.	M. V.	No.	Movement.	M. V.	No.
169	9	9	114	6	12

Series III. Disturbance Caused by the Sound of an Induction Coil and a Continuous Shock.

Reaction.	M. V.	No.	Movement.	M. V.	No.
173	18	13	120	6	13

Series IV. Normal.

Reaction.	M. V.	No.	Movement.	M. V.	No.
160	14	12	111	4	12

Series V. Same as Series II.

Reaction.	M. V.	No.	Movement.	M. V.	No.
166	17	12	114	7	13

Series VI. Same as Series III.

Reaction.	M. V.	No.	Movement.	M. V.	No.
172	13	12	125	3	13

SUBJECT B.

Series I. Normal.

Reaction.	M. V.	No.	Movement.	M. V.	No.
145	16	13	84	6	13

Series II. Disturbance Caused by the Sound of an Induction Coil.

Reaction.	M. V.	No.	Movement.	M. V.	No.
179	17	12	84	3	13

SUBJECT C.

Series I. Disturbance Caused by the Sound of an Induction Coil.

Reaction.	M. V.	No.	Movement.	M. V.	No.
164	17	12	87	4	13

Series II. Normal.

Reaction.	M. V.	No.	Movement.	M. V.	No.
151	18	13	83	4	13

Series I. Disturbance Caused by the Sound of an Induction.

Reaction.	M. V.	No.	Movement.	M. V.	No.
169	11	12	98	7	12

Series II. Normal.

Reaction.	M. V.	No.	Movement.	M. V.	No.
156	17	13	117	7	12

Series III. Disturbance Caused by the Sound of an Induction Coil and a Continuous Shock.

Reaction.	M. V.	No.	Movement.	M. V.	No.
172	8	12	113	8	13

[1] A very long reaction (266) if excluded here would bring this mean down to 159.

SUBJECT D.

Series I. Disturbance Caused by the Sound of an Induction Coil and a Continuous Shock.

Reaction.	M. V.	No.	Movement.	M. V.	No.
212	45	10	105	8	9

Series II. Disturbance Caused by the Sound of an Induction Coil.

Reaction.	M. V.	No.	Movement.	M. V.	No.
162	12	12	100	2	12

Series III. Normal.

Reaction.	M. V.	No.	Movement.	M. V.	No.
191	26	12	97	6	13

Series IV. Same as Series I.

Reaction.	M. V.	No.	Movement.	M. V.	No.
232	34	14	103	3	14

SUBJECT D.

Series I. Normal.

Reaction.	M. V.	No.	Movement.	M. V.	No.
170	16	13	93	7	13

Series II. Disturbance Caused by the Sound of an Induction Coil.

Reaction.	M. V.	No.	Movement.	M. V.	No.
165	13	13	95	3	13

Series III. Disturbance Caused by the Sound of an Induction Coil, and Continuous Shock.

Reaction.	M. V.	No.	Movement.	M. V.	No.
173	16	12	95	4	12

Section B. The Effect of an Intermittent Sound on Reaction Time and Movement Time.

The intermittent sound, as we have said, was caused by a metronome beating sounds.

SUBJECT B.

	Reaction.	M. V.	No.	Movement.	M. V.	No.
(Normal),	158	39	8	89	3	8
	166	28	11	91	3	11
(Normal),	164	30	9	93	3	8

SUBJECT D.

	Reaction.	M. V.	No.	Movement.	M. V.	No.
(Normal),	148	16	12	104	5	12
	240	30	9	112	4	11
	190	20	12	111	4	13
(Normal),	164	27	12	113	4	12
	179	20	11	105	3	13

Section C. The Effect of a Loud Signal for Reaction.

SUBJECT A.

	Reaction.	M. V.	No.	Movement.	M. V.	No.
I.	154	19	26	99	5	24
(Normal),	129	16	9	104	2	10

II.	153	19	34	106	3	25
(Normal),	180	27	10	121	5	11
	166	32	26	101	6	27
	158	17	38	98	4	37
		Subject B.				
				80	4	12
(Normal),	147	14	11	98	5	12
				100	4	12
(Normal),				93	1	18
		Subject C.				
				90	4	10
(Normal),	142	14	13	102	4	13

In examining these results it seems clear that when a sensory stimulus was given continuously during a series of experiments the reaction time was usually lengthened. This was most evident when the subject heard the sound of an induction coil, and also felt the shock by holding the electrodes in his left hand. The lengthening of the reaction time due to the sound of the coil alone was generally less than that caused by both the sound and the shock together. The effect on the movement time was not so marked; but it seemed that a continuous sensory stimulus tends to lengthen the time of movement. And, as a rule, the lengthening of the movement time — as of reaction time — was greater for the two disturbances than for one alone.

The effect of the intermittent sound was to lengthen reaction time, but no effect on the movement time could be ascertained. The meaning of this is that the intermittent sound acts merely as a disturbance of the attention.

The effect of the loud signal for reaction was without exception to quicken the time of movement. Its effect on the time of reaction was not constant.

11. The Graphic Curve of the Movement.

A complete study of the movement of reaction should give us not only the absolute time of the movement and its variation in a series of experiments, but also the variation in the speed movement throughout its entire course. Does the movement start with a maximum velocity and then gradually decrease? Or is the speed constantly accelerated? Does it vary irregularly? The best way of solving this question is of course to get

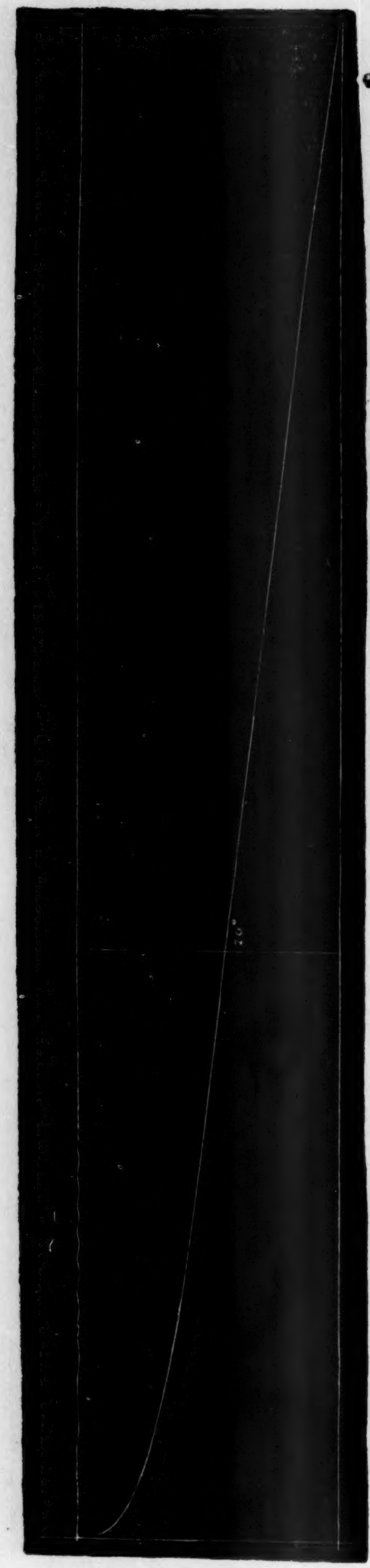


FIG. 11.

a graphic curve by which the movement will be represented in coördinates of time and space. To obtain this curve the following apparatus was used. A piece of heavy steel wire 104 cm. long and 2.1 mm. in diameter was arranged to run through two eye-holes, one in each of two brass rods supported on stands, beside the drum of a chronograph. This drum was 35.5 cm. long and 16 cm. in diameter. In the middle of the rod of steel wire a brass marker was soldered, which reached over to the top of the drum. The brass rods which supported the steel wire were so arranged that it ran through the eye-holes without friction, in a line parallel to the axis of the drum. When the drum was at rest and the steel rod was drawn through the eye-holes, it made of course a perfectly straight line along the top of the drum. When, however, the drum was in motion, a curve was drawn which varied with the speed of the drum and the rate at which the wire passed through the eye-holes. The speed of the drum, however, was nearly constant, and with the gearing and weights used it made sixty revolutions a minute. Through a small eye-hole in the end of the wire, a heavy linen thread passed

through pulleys to the lever which the subject moved in making the movement. To the other end of the wire a long, loosely wound steel spring was attached and passed backward to the wall. This served to keep the linen thread taut and make the wire follow the movement of the arm, whether it moved fast or slowly, backwards or forwards. Its pull against the arm, however, was too slight to have any appreciable effect on the movement. The drum being in motion, a graphic curve was obtained of whatever movement was made with the lever. A sample curve is here reproduced (Fig. 11).

In examining this curve, we find that during the first part of its course it is concave to a line drawn from the starting-point of the curve with the drum at rest. This shows that during this part of the curve, the movement was accelerated. It passes, however, into an approximately straight line before reaching the end of twenty degrees. This shows that at the point of transition the speed of movement attains its maximum and after this remains almost constant.

A number of curves were taken with the drum moving at a slower rate of speed, to see if the 'antagonistic reaction' noticed with some persons by Mr. Smith[1] would be found in the movements of our subjects. Only Subject *A* knew beforehand the purpose of the experiments. In these experiments the starting-point was a little away from the post to allow for any possible antagonistic movement. Though the slightest backward movement would have been recorded on the drum, no 'antagonistic reaction' was found for our subjects.

SUMMARY.

The empirical conclusions thus far reached may be summarized as follows:

1. When the subject is told to react as quickly as possible but nothing is said about the movement of reaction, whether it is to be fast or slow, there is no fixed relation between the reaction time and the movement time. The variations in one seem to be independent of those in the other.

[1] Cf. *supra*, p. 3.

2. When, however, the subject is told to react as quickly as possible with the quickest possible movement, the time of movement is practically constant, but the reaction time may vary considerably.

3. The idea of making the quickest possible movement has a tendency to make the subject react in the muscular manner.

4. There is no constant relation between the slight variations in reaction time and the larger variations in the movement time, or, in other words, the slower movements do not correspond to the longer reactions, nor do the slower movements correspond to the quicker reactions.

5. With some subjects this comparative constancy in the movement time is attained with little or no practice.

6. Varying the preparatory signal in successive series from one to three seconds causes the reaction time to change, but does not seem to affect the movement time.

7. When no preparatory signal at all is given, and when it is given at irregular intervals, the reaction time is considerably lengthened. The movement time is not lengthened, however, but is if anything a little shorter.

8. The disturbance of the attention caused by having the subject carry on a process of addition is in general greater than is compatible with executing the quickest possible movement.

9. In compound reactions, when the subject had to choose between a movement and no movement, the movement was generally quicker than in the normal simple reaction.

10. An exception was pointed out to the above conclusion which indicated also that the length of a compound reaction varies according to the direction of the attention.

11. There seems to be no considerable difference in time between a purely voluntary movement and that made in response to a stimulus for reaction.

12. The effect of a continuous noise made during a series of experiments by the interrupter of an induction coil was to lengthen the reaction time, and also to a slight extent the movement time.

13. The effect of this same noise plus a slight shock, received by holding the electrodes in the left hand, was to lengthen

still more the reaction time. In like manner, the movement made under the influence of two disturbances was as a rule slower than that executed during a single disturbance.

14. The effect of an intermittent sound, made by a metronome beating seconds, was to lengthen the reaction time, but there was no apparent effect on the movement time.

15. A very loud signal for reaction was followed by a quicker movement than that of reactions made with the usual tap of the hammer.

VI. THEORETICAL INTERPRETATIONS.

The conclusions we have just laid down are the condensed expression of the facts obtained in a number of experiments. It remains now to see what bearing they may have on questions of a more general character.

1. The Physiological Interpretation of Constancy of the Movement Time.

The first point which strikes our attention in these results is the variability of reaction time and the constancy of the movement time. In one and the same series the reaction time undergoes considerable change, but the movement time is fairly constant. If you introduce factors which increase the difficulty of attention, the reaction time is lengthened and rendered still more variable, but the time of movement remains about the same. Disturbances of the attention (within certain limits), therefore, lengthen reaction time but do not affect the speed of subsequent movement. If, now, we can find by what processes of reaction the movement is determined, we can argue that they are not affected by the fluctuation of the attention.

It is clear that the velocity of the movement is determined by the intensity of the motor discharge received in the muscles. Following this discharge back, we find that it is determined by the discharge of the root-cells in the grey matter of the spinal cord. Tracing the nervous impulse still further, it seems probable[1] that it received a certain amount of coördination in the bulb, to which it came from some higher center. And as we shall see later, this higher center is most likely the cortex. Constancy in the speed of movement would therefore be dependent upon constancy in the discharge of root-cells, invariably the same distribution in the bulb, and constancy in the discharge of the cortical cells. If we found that the speed of movement was lengthened by a disturbance of the attention, we

[1] Cf. Foster, 'A Text Book of Physiology,' 5th ed., New York, 1890, Part III., Section VI.

might infer — at least as far as logic is concerned — that either the cortical cells or the bulb or the root-cells of the spinal column were affected by the disturbance of the attention. For a slower movement would be due either to a weaker discharge of cortical cells or spinal root-cells, or to an improper distribution in the bulb, by which some of the discharge would be dissipated along the wrong paths. Since, however, in spite of the disturbances of the attention, the speed of movement remains constant, we must suppose that cortex, bulb and root-cells continue to function just as they do under normal conditions.

Now, this might be explained in two ways. In the first place, we might suppose that the disturbance of the attention actually does result in a relaxation of the maximum motor discharge. But before making the movement, the attention of the subject was again focused upon the idea of making the quickest possible movement and then by a new act of the will his maximum motor discharge was again prepared and sent forth to the muscles.

But against this view may be urged the conclusion from one set of experiments in compound reaction. In these experiments a choice was to be made between a movement and no movement. It was found that while the subject endeavored to react with the quickest possible movement, he executed a slower movement than in a normal simple reaction. The idea of not making a movement most likely brought about a relaxation of the preparation for the maximum discharge. A new act of the will was undoubtedly present in this compound reaction to execute the quickest possible movement; but it seems to have failed to do so. And there seems to be no reason to expect that after a disturbance of the attention the subject would, as it were, wait to re-establish the best conditions for a rapid movement and then execute it. Another idea also would be present in consciousness: to start as soon as possible. Without waiting for any complicated process to take place, the subject would react at once and make the movement — as in the set of compound reactions referred to — as best he could under the conditions. The processes of a new act of the will and re-preparation of the motor discharge complicate the process of reacting

too much. Some simpler explanation is probably the truer expression of what really takes place.

The other hypothesis which might be offered is that the subject can maintain the state of motor tension for the movement in the midst of such wanderings of the attention as occur when no preparatory signal is given and sometimes even while the attention is employed in such a process as addition. Of course a complete state of inattention would excessively lengthen the reaction time, during which period the maximum motor discharge might or might not be prepared for the execution of the movement.

However, the attention was by no means completely distracted during these series of experiments. Its wanderings were comparatively slight and we may say insufficient to disturb the motor preparation for the quickest possible movement. The central disturbance resulted in a lengthening of reaction time which was due to changes in the central stations or perhaps in the afferent paths. That the efferent path from cortex to muscle is not affected by the disturbance of the attention and that reaction time is not lengthened by any changed conditions along this path, seem to be the conclusions warranted by the fact of constancy in the time of the movement by which the reaction was executed. But can we apply the latter conclusion to reactions where there is no attempt to make the quickest possible movement? Have we not found that in such reactions the time of movement varies? But the variations in the time of movement when the subject does not try to make it fast or slow, are due to the varying intensity of discharge which the motor center happens to send forth — not of course to any varying resistance in the efferent paths. And there seems to be no reason to suppose that in one case attention would affect the efferent paths and in the other case it would not. Nor should we expect a lengthening of reaction time in the efferent circuit. The axones of the motor cells in the cortical area pass down through the pyramidal tract to the root-cells in the spinal cord without stopping at any relay station in their course. Once a motor discharge is sent forth it is hard to see how any delay could be experienced owing to a previous disturbance of the

attention. With the efferent circuit, however, matters are different. The path is made up of several superimposed neurones. And one could easily imagine that if the subject were not expecting the signal for reaction the connections between these neurones might be interfered with so that the stimulus would be delayed in its path to the center.

2. Physiological Interpretation of the Variations of the Movement Time Under the Influence of Sensory Stimuli.

If it be true that a mere disturbance of the attention does not interfere with the motor processes of reaction, then it must be admitted that the sensory stimuli, which seemed to lengthen the movement time as well as the reaction time, were probably more than mere distractions of the attention. It would seem that a continuous noise during a series of reactions in some manner either lessened the amount of potential energy at the disposal of the motor center or antagonized the maximum motor discharge after it has passed outward to the muscles. Perhaps the truest expression of what happened is that the sensory and motor centers are so closely connected that the motor center cannot do its maximum work while the sensory is continuously employed. In those experiments where the sound of the induction coil or the shock of the electrodes or both acted as disturbing influences, certain sensory centers were continuously at work throughout each series. This is one fact. The second is that the maximum discharge of the motor center was lessened. The supposition just suggested seems to be the mere statement of these two facts in theoretical terms. The only possible objection arises from the alternative already mentioned. Perhaps the center of the discharge is not interfered with at all. The sensory discharge flows over into a motor discharge which antagonizes that of the motor center for the movement. But why should that discharge be an antagonistic discharge? Why should it not flow along paths already open, and reinforce the movement? M. Féré found that working a set of muscles other than that employed on the ergograph reinforced the working finger. It is indeed hard to see why the motor discharge ac-

companying the continuous operation of the cutaneous and auditory centers should have an antagonistic effect upon the outward rotation of the arm, unless these centers were in some way closely connected with the muscles which give the humerus an inward rotation — a supposition for which there is no ground whatsoever.

It would seem more probable, therefore, to say that a continuous operation of the cutaneous and auditory centers lessens the amount of potential energy at the immediate disposal of the motor center for the arm.

The reinforcement of the movement by the loud signal for reaction is not against this view of the matter but brings out more clearly the close connection between the sensory and motor areas. When the subject is waiting to respond to the signal to react, we can readily suppose that the connections between the sensory and motor areas are already prepared. When the incoming stimulus is very loud, the auditory center is discharged with great force. The extra energy thus liberated passes over to the motor center, discharging it more forcibly and producing a quicker movement. The signal for reaction is necessarily associated with the motor area for the movement. We should therefore expect that the louder signal would reinforce the motor area. But the sound of the induction coil or the shock of the electrodes are not in any way associated with the movement. The sensory discharge of those centers does not therefore reinforce the motor area for the arm, but by maintaining a state of neural tension in the sensory cells in some manner lessens the maximum tension in the motor cells.

3. Bearing of Experiments on Professor Münsterberg's Action-Theory.

While one may admit that (as our experiments seem to prove) sensory and even intellectual representative processes have a tendency to flow over into movement, still it would be a further and bolder step to say that consciousness is absolutely dependent on the possibility of a motor discharge. One might with perfect logic admit the former and deny the latter as a conclusion too broad for the present basis of facts. The theory

that consciousness is absolutely dependent on the possibility of a motor discharge is associated with the name of Dr. Münsterberg. He has outlined his position in the last chapter of his 'Grundzüge der Psychologie.' The above discussions of the relations between the fluctuations of attention and the motor discharge in reaction are suggestive of certain objections which it may not be out of place to mention. But before doing so it will be necessary to give some account of the peculiar theory of which Professor Münsterberg may be regarded as the author, although in many of its features it is not new.

(*a*) *Outline of the Theory.*

Professor Münsterberg has called his explanation of consciousness the action-theory, because it makes use of motor processes in accounting for psychophysical phenomena. Previous theories have contented themselves with centripetal processes; the action-theory makes use of the centrifugal processes as well. According to the association-theory the orientation of consciousness (Stellungnahme) is dependent on purely psychical functions; but Dr. Münsterberg considers as fundamental the motor functions of the brain and even real actions of the organism. The new theory boasts of no recent physiological discovery as its protector. It hopes to win adherents on the field of battle. It starts out from the well-known facts of vividness and penetration, of reinforcement and inhibition, of furtherance and suppression, and associates with these the physiological processes of centrifugal action. To a certain extent it accords with the older theory of association. It admits that the quality of sensation is determined by the efferent path, and the quantity by the intensity of the incoming stimulation. But to explain its peculiar accompanying characteristics (Wertnuance) and the liveliness with which it affects consciousness, Dr. Münsterberg calls into his service the motor side of the process.

The very condition for any psychophysical phenomenon is the possibility of a motor discharge. When the afferent stimulus flows over into an efferent discharge it awakens consciousness to action. And it is this very act of passing which makes the subject conscious. If the proper motor discharge were

completely blocked, stimulus after stimulus might impinge upon the organ of sense and be carried to the brain, and still the subject would know nothing of the afferent process. That we may not be able to notice any motor disturbance as a result of conscious processes is no conclusive argument against the theory. Plethysmographic researches have pointed out the connection between psychophysical work and changes in blood volume; and the study of the knee-jerk has shown how remarkably the reflex centers are dependent on states of consciousness.

Turning to physiology for a closer view of motor processes, it is found that the cortex does not send its impulse directly to the muscle, but first acts upon certain subcortical centers. These in their turn may transmit the impulse to medullar ganglions. On this position of physiology the action-theory finds a foothold, and lays down the further statement that each subcortical motor center is connected with an antagonistic center. No central motor impulse is known which is not associated with an antagonistic discharge. The typical example of this is the relation between the muscles of flexion and extension. All antagonistic functions of the nervous system, all reinforcement and inhibition, rest upon this opposition of actions. "But there is no psychophysical event which as such is opposed to another psychophysical event, there are no two ideas which, as psychical images, exclude each other. There are no two sensations in whose very nature it lies that they cannot be simultaneously present in consciousness."[1]

It is on this account that any attempt to explain the phenomenon of reinforcement and inhibition from the relation between sensory processes is preordained to failure. "There is but one opposition, that which is based upon mechanical necessity. We cannot perform one action and at the same time execute the antagonistic, we cannot at the same time go to the right and the left, we cannot simultaneously raise and lower the eyes, we cannot at the same time breathe in and out, we cannot at the same time stretch out our hand and draw it back, in brief, only an action has its antithesis — an idea never; only an action can

[1] 'Grundzüge der Psychologie,' Vol. I., p. 534.

never be performed unless an antagonistic movement is thereby excluded; while any psychophysical stimulus can in itself be united with any other, no physiological reason can be found in the very nature of such a stimulus which makes the suppression of a coördinated sensory stimulus a necessity." The very foundation of the action-theory is based upon this conception. Sensations are lively and forcible when the sensory stimulus finds no resistance along the path of its discharge. But whence comes this resistance? From antagonistic motor centers. If the sensory stimulus passes over into the paths leading to a subcortical motor center, which is itself inhibited on account of a stimulus proceeding from the antagonistic center, the motor discharge meets with resistance. The reciprocal action of antagonistic centers accounts for the continuous play of reinforcing and inhibitory forces on the field of consciousness.

The action-theory is independent of any microscopical investigations in anatomy. It transcends them and rests upon an altogether higher plane. If metaphysics is only well founded, there can be no doubt that physiology and anatomy will, in time, suit their facts to the theories. But the action-theory can, with equal facility, fit into any of the more special hypotheses of nerve action founded on physiological and anatomical facts. As an example, the primitive conception of nerve action as a flowing of currents in and out of reservoirs can be adapted to this theory. And by doing so Dr. Münsterberg brings out the conceptions of the theory in a clear and tangible form.

According to this primitive conception of nerve action, "the outgoing current from the reservoir of the cortical cells, which flows towards the periphery through the axis cylinder, would come to a standstill if the lower level towards which it flows were full and had no outlet. But if the vent in the lower basin is open, so that the current can flow in the path to the muscle, then the stream can pour forth from the cortex. The opening of outgoing pipes in the lower center at once brings about a change in the upper reservoir. As long as the lower basin is blocked there can be no current in the upper basin, no matter how much fluid may pour into the upper reservoir from conducting paths. And if it is precisely upon the current that the

turning of the psychical millwheel depends, then the mill stands still when the lower basin is stopped up, and begins to rattle away when the sluices are opened below. We must now ask for but one more step — that each lower reservoir stand in antagonistic union with a neighboring one, so that the opening of the sluices in one automatically closes those in the other." [1]

Besides mere vividness, an impression is characterized by many other qualities, such as being desirable or undesirable, familiar or unfamiliar, etc. Can the action-theory offer any explanation of these ? It seems to find no difficulty in the way. There is more than one path possible for the motor discharge. In fact, there are innumerable possibilities. Just what discharge shall correspond to any given stimulus, is dependent on a very complex set of conditions of which we know almost nothing. But variations in the path of discharge give rise to those different characteristics which give the idea a certain shade of value in consciousness (Wertnuance).

To sum up the theory in a few words: "Each element of consciousness is coördinated with a transfer from stimulation to discharge in the cortex, and in such a way that the quality of the sensation depends on the spatial position of the path of stimulation, the intensity of the sensation on the strength of the stimulus, the accompanying characteristics (Wertnuance) on the spatial position of the path of discharge, and the vividness of the sensation on the strength of the discharge." [2]

If the action-theory is to be of any service in psychology it must give us an explanation of psychological phenomena. And it seems to be applicable with special facility to the theory of attention. "That remains unnoticed," says Professor Münsterberg, "for which an action is not prepared, till the strength of the stimulus forces the act; but on the contrary the attention lays hold of that for which the motor discharge is prepared. * * * That which opposes and hinders attention according to the action-theory is always and only what leads to antagonistic actions. It is the reciprocal blocking of the canals of movement which prevents the dissipation of attention." [3] If attention

[1] *Op. cit.*, p. 542–543.
[2] *Op. cit.*, pp. 548–549.
[3] *Op. cit.*, p. 550.

passes over into apperception this means that the stimulus is able to start up a more complex reaction than that which corresponds to its own isolated activity. This is the foundation for the distinction between apperception and perception. If our mind fixes upon any object it means that a stimulus arouses a certain type of action.

(*b*) *Criticism of the Theory.*

It seems probable that the experiments of this dissertation may throw some light on the theory of attention just outlined. In performing these experiments the idea to which the subject had to attend was to make the quickest possible movement on hearing a certain signal. If the signal comes when the subject is inattentive this must mean, according to Professor Münsterberg, that a certain path of movement which belongs to this idea is more or less blocked. What path of movement would this be? It seems that the motor path concerned in the movement of reaction — to which the idea corresponds — should be in great measure concerned. We should then expect in a series of reactions made under conditions favorable to attention that the maximum speed of movement would be greater than in a series where the attention is disturbed. For if the path of movement were more or less blocked, the full motor discharge could not pass along it, and the movement executed would necessarily be slower. Of course the movement of reaction is not the motor discharge which Professor Münsterberg speaks of as being the necessary condition for consciousness.[1] It comes as the result of far more complicated conditions than those which result in a motor discharge when an idea flashes into consciousness. But, notwithstanding this fact, the motor discharge passes along motor paths most likely to be followed by the discharges of those conscious processes concerned in reaction. If the idea of making a movement as quick as possible has any motor discharge, the path of discharge should be in great measure that concerned in making the movement. And if inattention blocks this path of movement we should certainly expect that the movement executed would be slower in consequence of the greater resistance to the motor discharge.

[1] Cf. *op. cit.*, p. 539.

In answer to this line of argument it might be said that the movement is executed only after the attention is focused, that the delay in reaction time means a reopening of the proper motor path, and that the movement then executed takes place as in a normal experiment. But such an answer seems to suppose the older theory of attention, which regards it as a kind of faculty, on which the motor processes may depend, rather than that attention depends on motor processes. When the inattentive subject hears the signal, there is a flash of consciousness— 'now's the time' — and the movement is executed. That flash of consciousness represents the focusing of the attention upon a rather complicated idea — to make the movement at once and as fast as possible. It was not the return of the more general idea to be ready to make the movement as fast as possible, as soon as the signal might be heard. It was a special determinate idea on which the movement followed as a matter of necessity. According to Professor Münsterberg, the flash of this idea into consciousness meant that a motor discharge had already taken place, that a certain type of action had been aroused. It passed along motor paths just as it happened to find them more or less blocked by previous conditions. Would not this discharge pass through the very subcortical centers and along the very paths concerned in the movement of reaction? And would not the movement of reaction be executed at the same time? And if this be true, any resistance encountered along this path (which, according to Professor Münsterberg, corresponds to a disturbance of the attention) will delay the reaction and lessen the speed of movement. But under conditions which were unfavorable to attention we found that the movement time was not lengthened, though the reaction time was considerably slower. Nor did the longer reactions correspond to the longer movements which occurred within the small limits of variation in the movement time.

It must be acknowledged, however, that the test we have applied cannot be considered as final. Perhaps the criticism must be considered as premature, for Professor Münsterberg has not yet made his theory sufficiently explicit. It still rests in the shades of metaphysics, for it has not yet ventured forth into the

broad day of experimental science. If it should do so in the future it cannot yet be said in just what form it will appear.

But apart from this criticism suggested by the experiments, there are other points which, having once opened the subject, it may not be out of place to mention.

The first is a point of method. Dr. Münsterberg would place his theory far above all microscopical investigations. It is not for him to conform his theory to the facts of anatomy and physiology. Let the anatomists and physiologists be subject to the theory. Put forward a theory and let others look about for the facts to support it, seems to be the principle on which he has proceeded.[1]

Such a method refuses to take cognizance of the lesson taught by the history of modern science. Descartes' vortex theory to explain the motions of the planets was based on the metaphysical speculations; Newton's theory of gravitation was built upon calculations made from carefully observed facts. The former is now an historical curio, the latter still forms the basis of all astronomical calculation. This is but one instance of many which could be adduced as examples of the success of the inductive method of investigation, where speculation has failed. Can Professor Münsterberg boast of such special powers of penetration that he can afford to neglect the lesson taught by the discoveries of modern science? It must not be supposed, however, that I absolutely deny the value of metaphysical inquiry. It has its own field, and there purely inductive reasoning may be of little or no worth. But whenever facts are forthcoming, these must serve as the basis of speculation. And a theory which deals with subcortical motor centers, afferent and efferent paths, cortical and spinal cells, cannot plume itself upon being above the domain of facts, but must yield place to them and not go further than they allow.

The next point of criticism may seem to be one of mere words. But in speaking of the cortical motor discharge, Professor Münsterberg uses terminology which is more than technically at fault. It seems to be the expression of fanciful and improbable ideas. "Now physiology," he says, "gives us the

[1] Cf. *op. cit.*, p. 406, p. 530, p. 540.

further information that the cortex does not send its impulse directly to the peripheral muscles, but first acts upon subcortical centers, which in their turn send the impulse down to medullary ganglia."[1] What these subcortical centers are, or where they are situated he does not say. On the next page he seems to locate the medullary ganglia in the anterior horn of the spinal cord. From this it would seem that Professor Münsterberg divides the path for conduction of motor impulses into three stages, viz., (1) from the cortex to subcortical center; (2) from the subcortical center to the root-cells in the anterior horn of the spinal column; (3) from these root-cells to the muscle. He gives special prominence to the subcortical centers when a little later on he says that, 'the cerebral cortex, from which alone psychophysical stimuli flow forth, must work upon subcortical centers to discharge motor impulses.'[2] This statement, however, is untenable. According to all anatomists the fibres of the pyramidal tract pass directly from the Rolandic area down through the inner capsule to the crura and (omitting for the sake of simplicity the cranial nerves) thence through the medulla oblongata to the spinal cord, terminating in various places along its entire extent. Perhaps the term *center* could be applied to a nucleus. There are several nuclei and, if you will, centers in the afferent paths. But the pyramidal tracts, either direct or crossed, are nowhere interrupted by nuclei. Nowhere along either of these tracts is there a motor center on which the cortex must act in executing a movement. There is most probably another path which the cortex may employ in executing a movement, and this path does involve a subcortical motor center. It seems to be very well established that there is a much more complex motor path from the cortex to the pons, from the pons to the cerebellum and thence downward to the anterior roots *via* the antero-lateral descending tract.[3] But just what its function is cannot be said. But that the primary path of motor impulses is the pyramidal tract is the general position of anatomists and physiologists. And this path is certainly not interrupted by any subcortical motor centers.

[1] *Op. cit.*, p. 532.

[2] *Op. cit.*, p. 533.

[3] Cf. Van Gehuchten, 'Anatomie du Système nerveux de l'Homme,' Vol. II., pp. 445-461.

Starting from the supposition that there are subcortical motor centers along the path of movement, Professor Münsterberg supposes two kinds whose action is reciprocal. One is bound to the other as to an antagonistic center, so that the stimulation of one subcortical center calls forth at the same time an inhibition of the antagonistic center. In support of this view he cites the experiments of Dr. Sherrington on the reciprocal action of antagonistic muscles. Dr. Sherrington has shown that for some muscles, and under certain conditions, a contraction of the extensors is accompanied by a relaxation of the flexors and *vice versa.* But Dr. Sherrington does not localize the centers for reciprocal action. He certainly does not postulate any subcortical center on which the cortex must act — thereby inhibiting one antagonistic center — and so executing the movement. Dr. Sherrington enumerated[1] the places of excitation where the phenomenon of innervation could be elicited. These were:

1. "The skin and skin nerves (with 'decerebrate rigidity');
2. The muscles and the afferent nerves of muscle (with 'decerebrate rigidity');
3. The dorsal (posterior) columns of the cord (with 'decerebrate rigidity');
4. Of the cerebellum (with 'decerebrate rigidity');
5. Of the crusta cerebri (with 'decerebrate rigidity');
6. Of the internal capsules;
7. Of the optic radiations;
8. Of the Rolandic cortex;
9. Of the occipital (visual) cortex."

But in all this there is no warrant for the conception of Dr. Münsterberg, that the cortex acts upon subcortical centers reciprocally connected, that one of these is inhibited and the antagonistic muscles relaxed, and the other discharges the root-cells of a set of muscles which contract. The fact that under certain conditions the phenomenon is called forth by excitation of the skin and skin nerves, afferent nerves of the muscles, and the dorsal columns of the cord, would indicate that 'reciprocal innervation' is of a reflex character, taking place perhaps at

[1] *Proc. of the Royal Society of London*, Vol. LXII., p. 187.

various places along the spinal cord or in the bulb and due to afferent impulses from the contracting muscles. Professor Münsterberg's postulation of reciprocal subcortical motor centers, which divide the pyramidal tract into two portions, is superfluous and contrary to the facts known to anatomical science.

4. The Motor Center Employed in the Process of Reaction.

Another point on which the results of these experiments bear is the question concerning the motor center employed in the process of reaction. This has been the result of no little theorizing. It originated before the distinction between sensory and muscular reactions was brought out. In an article published in *Philosophische Studien*[1] in 1886, Cattell put forward the view that with practised subjects a reaction was probably carried on without aid of the cortex. He wrote as follows: "In the same way a reaction such as we are considering can probably be made without need of the cortex, that is, without perception or willing. When a subject has had no practice in making reactions (in which case the reaction time is usually longer than 150 σ) I think the will-time precedes the occurrence of the stimulus. That is, the subject by a voluntary effort, the time taken up by which could be determined, puts the lines of communication between the center for simple sensations (in the *optic thalami* probably) and the center for the coördination of motions (in the *corpora striata*, perhaps, connected with the *cerebellum*), as well as the latter center, in a state of unstable equilibrium. When, therefore, a nervous impulse reaches the *thalami*, it causes brain changes in two directions; an impulse moves along the cortex, and calls forth there a perception corresponding to the stimulus, while at the same time an impulse follows a line of small resistance to the center for the coördination of motions, and the proper nervous impulse, already prepared and waiting for the signal, is sent from the center to the muscle of the hand. When the reaction has often been made, the entire cerebral process becomes automatic, the impulse of itself takes the well-traveled way to the motor center and releases the motor impulse."[2]

[1] Vol. III., p. 322. Cf. also *Mind*, 1886, Vol. XI., p. 232.

[2] *Mind*, *loc. cit.*

When Ludwig Lange, in 1888, published his article on the two kinds of reaction, he put forward the hypothesis that the motor center for muscular reaction is located in the cerebellum. It did not seem to him possible that there should be a special act of will involved in the actual process of muscular reaction, because the subject is not conscious of any new voluntary impulse in responding to the given stimulus. He looked upon the muscular reaction as a brain reflex which differs from the reflex actions of lower centers solely because it requires a preceding stimulus of the will to make the necessary preparation for executing the movement. This idea he made more explicit by further details. The will causes to be stored up in the subcortical center a certain amount of potential energy, which is held there in unstable equilibrium. A sensory stimulus coming into this center disturbs the equilibrium, and the energy thus set free flows over along motor paths to the muscle and is there manifested in the executed movement. This hypothesis can account for the previous tension of the muscles, the fact that muscular reactions are frequently made in response to the wrong stimulus, and certain other phenomena which accompany the shorter form of reaction.

But on what anatomical grounds can it be stated that the cerebellum is the subcortical center for muscular reaction? "As far as the cerebellum is concerned there is indeed established with anatomical certainty only —

(*a*) The immediate connection with the sensory fibres of the direct cerebellar tract; this would give sufficient room for conduction from the organs of touch;

(*b*) Connection with the motor regions of the cortex (Kleinhirnbrückenbahn), *i. e.*, with the center of voluntary muscular innervation;

(*c*) Connection with the sensory region of the cerebral cortex (Kleinhirn-Grosshirnbahn), *i. e.*, with the central sensory surfaces.

There can be added as very probable:

(*d*) A motor path to the spinal column. In this would be had the requisite connection with the group of reacting muscles.

In order to refer to the cerebellum muscular reactions for all the domains of sense, over and above these connections, there must be supposed:

(*e*) A sensory connection between the cerebellum and the optic nerve.

(*f*) A similar connection with the acoustic nerve.

Both these conducting paths are probably present, according to what is now supposed concerning the functions of the cerebellum."[1]

What Cattell thought was an analysis of the central processes of reaction in general, Lange claimed could be applied to the muscular form of reaction alone. He saw no reason to depart from Wundt's analysis of reaction when applied to the sensorial form.

Dr. Sigmund Exner, in his 'Psychische Erscheinungen,'[2] after referring to this view of Lange's, says: "There can be no doubt that this form of a subtle voluntary movement is to be referred to this, that the intention of the will to execute a special movement as quickly as possible in response to the given stimulus, rests upon a change which occurrences in the cortex call forth in the conditions of irritability in the subcortical centers. The condition thus brought about, which arises voluntarily, then occasions without a new conscious act that the entrance of a stimulus should produce a movement." Though Dr. Exner here commits himself to the position that the muscular form of reaction is carried on by a subcortical motor center, he does not specify any particular center.

In reference to Lange's position, Wundt writes: "Ludwig Lange has put forward the conjecture that the transmission of the sensory into a motor discharge within the cerebral cortex takes place only in the sensorial reaction; in the muscular reaction, however, it takes place in a lower center, probably in the cerebellum, or possibly in the ganglia of the midbrain. I do not believe that the arguments brought forward for this position are convincing, or that they even make the hypothesis advanced probable. Lange was certainly right if he held it was exceedingly probable that in muscular reaction the transfer takes place in a lower center, or that, at any rate, factors are wanting in it which in the sensorial reaction proceed from a higher center, standing in close relationship with the impulses of the will.

[1] *Philosophische Studien*, Vol. IV., pp. 517-518.

[2] P. 158.

But doubtless there are centers of different order in the cerebral cortex. If the muscular reaction takes place almost at the same time as the entrance of the sensation, then there is nothing in the way of supposing that the transfer takes place where the conscious sensation is discharged — in the primary sensory centers of the cerebral cortex. The delay present in sensorial reactions which prevents erroneous reactions, very early appeared to me to call for the hypothesis of an inhibitory action proceeding from an apperception center, which lasted until the stimulation (Signalreiz) belonging to this center caused a partial discharge of it. It also seems to me that the presence of transitional forms can best be harmonized with this view. For it is easy to understand that such an inhibition can be more or less active; but it is very difficult to see that between a reflex discharge, merely in a subcortical center, and a function of the cerebral cortex, such transitional values could be present."[1]

The evidence from our experiments seems to point to the conclusion that the centers for muscular reaction are not so widely separated as the cerebral cortex and the cerebellum. On the hypothesis that there is a distinction between sensorial and muscular reactions, it is quite clear that those made without a preparatory signal were sensorial reactions. For leaving the subject without a preparatory signal makes it impossible for him to attend primarily to the movement. He must be straining his attention to catch the sound of the reaction stimulus. On the other hand, the reactions made by the subjects when the preparatory signal was given were, as we have said, mainly of the muscular form. This was particularly true of Subject *B*. But we found no decided difference between the speeds of movement in the two cases. Perhaps the speed of movement in the reactions without preparatory signal was a little quicker than in the muscular reactions. But this was not constantly the case, and was probably due to the factors already mentioned.

Then, again, we compared the speed of the movement which came as the result of a voluntary choice with that which followed upon a reaction. In the experiments by which this

[1] 'Grundzüge der physiologischen Psychologie,' 4th ed., II., p. 317. Cf. also 5th ed., III., p. 428.

comparison was made the subjects probably reacted in the muscular manner. But there was no decided difference in time between the voluntary movement and the movement of reaction.

It will be generally conceded that the motor center for the voluntary movement and the sensorial reaction is situated in the cortex. And at the same time Lange's conception of the process of muscular reaction cannot be shown to be impossible, even after the great advances made in the anatomy of the nervous system since 1888. But if we suppose that the center for muscular reaction is subcortical, it would seem likely that the line of movement executed by the center would differ from that of the movement executed by the cerebral cortex — and for two reasons. In the first place, if a subcortical center is reflexly discharged by the incoming stimulus in a muscular reaction, it is probable that the intensity of this discharge would be different from that of a discharge coming from the motor area of the cortex. The root-cells in the spinal cord would therefore be discharged with different force in each case and the subsequent movements would differ in speed. And in the second place, even though the normal maximum discharge of the cortical cells and the subcortical center might happen to be equal, it is still very unlikely that the discharges passing from the cortex and subcortical center would be so equally distributed that the final resultant which reached the root-cells of the spinal cord would be the same in the two cases. The path from the cerebral cortex to the spinal root-cells is without interruption along the fibers of the pyramidal tracts. If, for instance, the subcortical center were the cerebellum, the sensory stimulus would reach it through different nuclei along a different path, and then when the motor cells of the cerebellum were discharged the impulse would not pass to the root-cells by the pyramidal tract, but probably by the antero-lateral descending tracts. Different connections would have to be made in the grey matter of the spinal cord from those used by the pyramidal tract. Such a difference in the path of movement would probably result in a very different distribution of the motor discharges, which would be manifested in the record of the movement time. Not finding evidence of such a difference, we conclude that the

center for the different kinds of movement cannot be so widely separated as the cortex and a subcortical center. It seems most reasonable to suppose that the motor cells concerned are those of the Rolandic area.

5. The Distinction between Muscular and Sensorial Reactions.

Now something must be said of the validity of the distinction between muscular and sensorial reactions. On this point there are two very opposite opinions. That of the Wundtian school may be expressed in Wundt's own words: "Um möglichst vollständige Reactionszeiten zu erhalten, muss die Aufmerksamkeit intensiv auf den erwarteten Sinneseindruck gerichtet werden, wobei sich die Spannung der Aufmerksamkeit immer zugleich durch Muskelempfindungen des betreffenden Sinnesgebiets, z. B., in den Accommodations -und Augenmuskeln dem Tensor-tympani verräth; dagegen darf sich die Aufmerksamkeit nicht aus das reagirende Bewegungsorgan richten, und das zuverlässige Kriterium für die Erfüllung dieser Bedingung liegt darin, dass die Muskelspannungen dieses Organs unmerklich sind. Will man dagegen einen extrem verkürzten Reactionsvorgang erhalten, so ist es nöthig die Aufmerksamkeit ausschliesslich auf das reagirende Organ zu verrichten, was immer mit einer intensiveren Muskelspannung desselben verbunden ist." [1]

The ground for the distinction, according to Wundt and his school, is the manner of directing the attention. All subjects who can at will place the strain of attention in the afferent or efferent circuit will be able to react either sensorially or muscularly.

Professor Baldwin, however, in propounding the type-theory of simple reaction sought another ground for the distinction. According to him, the root of the distinction lies in individual differences of the subjects. "We find," he says, "cases of relatively shorter sensory times similar to mine

[1] 'Grundzüge der physiologischen Psychologie,' II., p. 309 (4th ed.). Cf. also a somewhat different expression of the distinction in the 5th ed., III., p. 412 ff.

reported (for electrical stimulus) by Cattell, and (for sound stimulus) by Flournoy. We may accordingly say that such individual differences are clearly established, and must hereafter be acknowledged and accounted for in any adequate theory of reaction. The attempt of Wundt, Külpe, and others to rule these results out, on the ground of incompetency of the reagents, is in my opinion a flagrant *argumentum in circulo*. Their contention is that a certain mental *Anlage* or aptitude is necessary in order to experimentation on reaction times. And when we ask what the *Anlage* is, we are told that the only indication of it is the ability of the reagent to turn out reactions which give the distinction between motor and sensory time which Wundt and his followers consider the proper one. In other words, only certain cases prove their result, and these cases are selected because they prove that result. It is easy to see that this manner of procedure is subversive both of scientific method and of safely acquired results in individual psychology. For the question comes: What of these very differences of individual *Anlage?* How did they arise; what do they mean? Why do they give different reaction time results? To neglect these questions and rule out all *Anlagen* but one, is to get the psychology of some individuals and force it upon others and thus to make the reaction method of investigation simply the handmaid to dogma."[1]

"The individual differences in reaction arise from the fact that there is a natural and permanent tendency in all people to take the same cue for their movements that they do in speech. The doctrine of 'types' rests upon certain facts which may be briefly summed up. A voluntary motor performance — say speech — depends in each particular exercise of it upon the possibility of getting clearly in mind (*interieur*, *innerlich*) some mental picture, image, presentation, which has come to stand for or represent the particular movements involved. This mental 'cue' or representation may belong to either of two great classes: it may be a 'sensory' cue or a 'motor' cue. People are of the sensory type, or of the motor type for speech, according as their cue in speech is sensory or motor, that is, according

[1] 'Types of Reaction,' J. Mark Baldwin (and W. J. Shaw), PSYCHOLOGICAL REVIEW, 1895, II., p. 265.

as in speaking they think of the sounds of the words as heard, the looks of the words as written, etc. — the cues furnished by the special senses associated habitually with speech — this on the one hand; or according as, on the other hand, they think of or have in mind the movements of the vocal organs, lips, tongue, etc., involved in speech."[1] "So in simple hand movements people may show the sensory and motor types. This is my hypothesis. The man, therefore, who gives relatively shorter motor reactions is a 'motor' in his type; with him the thought of movement is the most facile beginning of the movement, just because it is really the movement and nothing else that he thinks of. That is his *Anlage*. But the man who gives relatively shorter sensory (auditory, visual) reactions is a 'sensory' in his type; with him the attempt to think of the movement *as a movement* interferes with the prompt and exact execution of it, just because he is not accustomed to execute his movements in that way. That is his *Anlage*."[2]

It would be out of place in our present paper to criticise extensively either of these theories of reaction, since we have made no special test concerning them. But it does seem incumbent upon us to state what was meant in the course of the paper when the terms sensory and muscular reaction were used.

We have supposed with Wundt and his school that when the strain of attention (Spannung) is really in the efferent circuit all subjects will react more quickly than when the strain of attention is in the afferent circuit. But the attention here required is of the nature of tension, or what the Germans call *Spannung*. Only secondarily is it a visualization of the movement or of the signal. However, Professor Baldwin's experiments do present a real difficulty to this position, and it would not be fair to ignore them or arbitrarily rule them out of court. They are facts, and as such they must be accounted for. But their ultimate explanation may not be in the theory of types as expounded by Professor Baldwin. Some subjects find it hard to direct their attention to the movement, and others find difficulty in directing their attention to the signal. Perhaps the

[1] *Loc. cit.*, p. 268.
[2] *Loc. cit.*, pp. 269–270.

reason for this is to be sought in the fact that some are in the habit of attending to sensory images and others to motor images. But when a subject who is really of the motor type tries to react sensorially and gives a short reaction, he may indeed picture to himself a sound or a light, but it might be said that the attention or *Spannung* was not primarily in the afferent, but as a matter of fact in the efferent circuit. But at the same time, if he commenced to think about the movement, to try to see it or feel it, what was before carried on automatically might be interfered with by being consciously attended to. Tension would be drawn away from the muscles, to be given to visualizing the movement. In every reaction there are two mental images which fluctuate more or less in consciousness while the subject is waiting to react. One is the sensory representation of the signal; the other is the motor (or perhaps visual) representation of the movement. But while this fluctuation of mental images is going on there is not a simultaneous changing of the center of neural tension. Too little attention has been given to this distinction — between Spannung and Aufmerksamkeit — in previous discussions of the problem. Perhaps a better understanding of the distinction might show that the two theories are really closer together than they seem to be at present.

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[1] The literature which bears on the stimuli which affect the knee-jerk should be considered here. Most of it will be found in the bibliography appended to Dr. Sternberg's monograph, which contains over eight hundred references.